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# ESSAYS
## CONCERNING
# IRON AND STEEL:

THE FIRST,

Containing OBSERVATIONS on
AMERICAN SAND-IRON:

THE SECOND,

OBSERVATIONS, founded on Experiments, on Common IRON-ORE, with the Method of reducing it firſt into PIG or SOW-METAL, and then into BAR-IRON; on the Sort of Iron proper to be converted into good STEEL, and the Method of refining that Bar-Steel by Fuſion, ſo as to render it fit for the more curious Purpoſes: With an Account of Mr. REAUMUR's Method of ſoftening Caſt-Iron;

AND

AN APPENDIX,

Diſcovering a more perfect Method of Charring PIT-COAL, ſo as to render it a proper Succedaneum for charred WOOD-COAL.

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By HENRY HORNE.

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LONDON:
Printed for T. CADELL, in the Strand.
M.DCC.LXXIII.

RPJCB

At theſe words, p 17. [I ſhall not fail communicating them to you] put an aſteriſm, and add the following note:

The phænomenon here referred to is, that, whereas I expected it would turn out hard pig-metal, it in fact turned out tolerably malleable ſteel; which was the caſe alſo in various trials upon all kinds of mine-ſtone. But concerning this matter I need ſay no more in this place, as it is ſufficiently conſidered in the Second Eſſay; except only juſt to remark the difference, which muſt of neceſſity happen between an experiment made upon a ſmall quantity of ore in a crucible, where the external air can have no admiſſion, and that made in a large ſmelting-furnace by the violent blaſt of an enormous pair of bellows. In the former caſe, the efficacy of the charcaol, mixed up along with the ore, is diſpenſed in a ſlow and nearly inſenſible manner; whereas in the latter, both the ore and the coal are almoſt inſtantly torn to pieces, and the ſalt and ſulphur before locked up in the charcoal are as ſpeedily ſet at liberty, and powerfully excited to execute their office. So that in the latter caſe, it is not at all ſurprizing, that the metal ſhould be much harder and more fragile, than in the former; but in both caſes, it is extremely evident, that the ferrugineous atoms are fairly converted into ſteel.

JOHN CARTER BROWN

# OBSERVATIONS

ON

# AMERICAN SAND-IRON.

In a LETTER to

The late Mr. JOHN ELLICOT,
F. R. S.

Read at the ROYAL SOCIETY,
March 3, 1763.

# OBSERVATIONS
## ON
# AMERICAN SAND-IRON.

To Mr. JOHN ELLICOT.

SIR,

AS the affair of the rich American Iron Ore, commonly known by the name of the Virginia black ſand, has of late not only engaged the converſation of many of the Virtuoſi, but has been taken very particular notice of by the Society for the encouragement of arts and manufactures; I thought myſelf obliged, for many reaſons, to lay before you whatever has come to my knowledge relating to this diſcovery, either from my own experiments, or from the information of others. And I engage in this ſervice with the greater pleaſure, as I look upon it to be one of the moſt intereſting diſcoveries, with regard to this uſeful metal, that has come to our knowledge for ſome ages, and, if rightly conducted, may prove of infinite ſervice to us in this part of the world, as well as to the inhabitants of our colonies, where (as it has been ſuppoſed, though without ſufficient foundation) this diſcovery was firſt made.

Without any farther preface or apology, permit me to remind you, that, in a converſation which formerly paſſed between us upon this ſubject, I acquainted you, that, about twenty years ſince, I was engaged in making a variety of experiments upon the nature of Iron-Ores, and Steel; and that I then made a very particular enquiry into the nature of this black ſand, and, in the courſe of theſe experiments, ſeveral very intereſting phenomena diſcovered themſelves, which, as they might be of great ſervice to the world in general, and more eſpecially to ſuch as are concerned in ſmelting of the iron from the ore, I had thoughts of communicating to the public; but, as my buſineſs will not permit me to go through the whole at preſent, I ſhall confine myſelf to what relates to the black ſand.

I procured, from Mr. Adams the Virginia merchant, a ſufficient quantity of the ſand, and, in order to eſtimate its comparative weight with that of iron ore, I procured ſome of the richeſt ore I could get, which having reduced to powder, I filled an ordinary tea-cup with it. I afterwards filled the ſame cup with ſome of the ſand, and upon comparing the weights with each other, I found that the weight of the ſand was to that of the ore as 3 to 2; and having taken notice how readily the ſand was attracted by the magnet, I was convinced that the ſand muſt certainly contain a very conſiderable quantity of Iron, and therefore determined to make trial of it. I was however, for ſome time, interrupted in my deſign, by information I received from a friend, that ſuch an enquiry had been made many years before, by a member of the Royal Society, and a gentleman of eſteem as a chemiſt, but without ſucceſs; and that the experiments were publiſhed in the 2d vol. of Lowthorp's Abridgment of the Philoſophical Tranſactions. As this account is very ſhort as well as curious, I ſhall take the liberty to give it you entire, with ſome few remarks upon it.

" A black ſhining ſand from Virginia examined by Dr. All. Moulen.

A ſmall vial filled with ordinary white ſand, and containing only ʒ i. gr. xi. being filled with the Virginia ſand, was found to contain ʒij. ℈ij. gr. i.

This ſand did apply to the magnet both before and after calcination; but the latter did apply better to it than the former.

A parcel of this ſand, mixed and calcined with powdered charcoal, and kept in a melting furnace for about an hour, yielded no regulus: but applied more vigorouſly to the Loadſtone than either of the former.

I fluxed a parcel of this ſand with fixed nitre, in a melting furnace, for above an hour; but could obtain no regulus; nor any ſubſtance that would apply to the magnet, except a thin cruſt that ſtuck firmly to a piece of charcoal that dropt into the crucible when the matter was in fuſion.

I fluxed it alſo with ſalt-petre and powdered charcoal; dropping pieces of charcoal afterwards into the crucible. It continued about half an hour in the melting furnace in fuſion, and that without producing a regulus, or a ſubſtance that would apply to the magnet, excepting only what ſtuck to the charcoal as in the former experiment.

I fluxed another parcel of it with ſaltpetre and flower of brimſtone, without being able to procure any regulus.

I poured good ſpirit of ſalt on a parcel of this ſand, but could obſerve no luctation thereby produced.

I poured ſpirit of nitre, both ſtrong and weakened with water, on parcels of the ſame ſand, without being able to diſcover any conflict.

I poured ſingle aqua fortis upon another parcel of it, without being able to perceive any ebullition worth noting.

I uſed alſo double aqua fortis upon another parcel of it, which, for ought I could diſcover, had no more effect on it than the former.

I poured ſome aqua regia on a parcel of it, without diſcovering any ſenſible effect. I poured good oil of vitriol upon another parcel of this ſand; but ſeeing no bubbles thereby produced, I weakened the oil with water, but without any viſible effect.

I repeated all the former experiments with the menſtruums upon this ſand after calcination per ſe in a crucible, but could ſcarce obſerve a bubble produced by any of them.

I poured ſome of each of the liquors upon parcels of the powder of this ſand calcined, without any ſucceſs.

Note, That I made theſe experiments both in the cold, and upon a ſand furnace. So that to me there ſeems to be but little wanting to diſcover any metal known to us, if it contained any ſuch: for there is no metal nor ore that ſome of theſe menſtruums will not work on.

I powdered a fragment of a loadſtone, and poured ſome of theſe menſtruums upon it, without being able to find that they in the leaſt preyed upon it, any more than they did upon the ſand.

I poured ſome of the aforementioned menſtruums upon ordinary ſand taken out of a ſand furnace, where it muſt have ſuffered ſome calcination; but could find no more bubbles produced thereby, than what might rationally be ſuppoſed to be produced from lime, and other dirt mixed with the ſand."

Having thoroughly conſidered theſe experiments, they appeared to me far from being deciſive, and that if the Doctor had placed more confidence in the power of the magnet, and leſs in his menſtruums, he would rather have concluded that there might be ſome ſorts of iron ore which his menſtruums would not touch in the moiſt way, nor any regulus be produced from them in the dry, as he made uſe of them, which yet might, under ſome other hands, be ſubdued, by more apt and powerful methods than any which at that time he was acquainted with.

However I apprehended I might fairly draw this concluſion from his experiments, viz. that the ſand was not altogether and ſimply iron, but that it was ſtrongly united with a very ſtubborn, fixed, and permanent earth, which could not be ſeparated from it without ſome extraordinary, as well as powerful means; but I could not think this a ſufficient objection to the proſecution of an experiment, which, if it ſucceeded, might be attended with very happy conſequences. Proceeding therefore upon this ſuppoſition, I mixt up about 8 or 9 ounces of the ſand, with a proportional quantity of a ſtrong corroſive flux, which I put together in a crucible, and committed it to a very ſtrong fire in an excellent wind-furnace, where I kept it for between two and three hours, hoping by this means to have anſwered the intended purpoſe; but I confeſs I was not a little ſurpriſed, that, after the crucible was taken from the fire, I could not find one ſingle grain of metal in the remaining contents.

This diſappointment greatly puzzled me, till having thoroughly examined into the unexpected event, without being able to diſcover any reaſon ſufficient to incline me to recede from my former opinion, as to the component parts of the ſand, I concluded that the flux might poſſibly be a very improper one; for though it might have effected the intended ſeparation, yet it might at the ſame time be ſufficiently powerful to divide the particles of the metal, when ſeparated, ſo very minutely, as to be capable of ſubliming and carrying them off imperceptibly: And finding the contents greatly diminiſhed, ſo that the quantity remaining bore but a ſmall proportion to that which was firſt put into the crucible, I concluded that this muſt really have been the caſe, and that ſome very different method muſt be purſued in order to produce the deſired effects. I immediately determined to make a ſecond trial, in which I proceeded in the following manner. I took the ſame quantity of ſand made uſe of in the former experiment; and firſt I ſpread it, without any addition to it, upon an iron plate over a ſtrong fire, where I gave it a very powerful torrification (or roaſting), to try if, by that means, I could not relax, and looſen the component parts to ſuch a degree, as to make the ſeparation and reduction of the metal more eaſy, when I ſhould bring it into the furnace. When I had ſo done, I mixed it up with a flux of a very peculiar, but gentle nature, which I had before made uſe of for other purpoſes with great ſucceſs, and committed it (as in the former experiment) to the furnace, where I urged it by a very ſtrong fire for about three hours, and upon taking it out, I found the event anſwerable to my moſt ſanguine expectations: for in the bottom of the crucible I found, as near as I can remember, rather more than half of the ſand I put into the crucible reduced to a very fine malleable metal.

In this very agreeable experiment I met with a very ſurprizing phænomenon, which, as I am not at preſent able to determine whether it was only caſual, or what would always happen in the like experiments, you will excuſe my divulging at preſent, eſpecially as you, Sir, by furniſhing me with a freſh parcel of the ſand, have enabled me to make ſome farther trials; which I ſhall embrace the firſt opportunity of doing; and ſhould I be ſo happy as to confirm what I then obſerved, or to make any farther diſcoveries deſerving your notice, I ſhall not fail communicating them to you.

Being fully convinced, by the experiment, that the ſand was a very rich iron ore, I acquainted ſome of my friends with it, who being largely engaged in trade to thoſe parts of our American colonies, where I was informed this ſand was to be eaſily procured, and in very large quantities, I was in great hopes an account of this nature would have inclined ſome of the gentlemen in that part of the world, to have proſecuted ſo uſeful a diſcovery in a larger way; and I own I have often wondered, that an affair of ſuch conſequence ſhould have lain dormant for ſo many years.

However I was a few months ſince pleaſingly ſurpriſed, to find in the hands of my very ingenious friend Mr. Peter Collinſon, not only a pamphlet, but likewiſe a letter upon the ſubject addreſſed to the Society for encouraging of arts and manufactures, by one Mr. G. Elliot, who relates, that, though previous to his attempt of making iron from this ſand, he met with nothing but what was diſcouraging from the moſt ſkilful perſons to whom he propoſed his deſign, yet that he had ſuch a perſuaſion in his own mind of the practicability of the thing, that he could not reſt till he had made a trial, and the event proved encouraging much beyond his expectations, inſomuch that he could ſcarcely believe the trial had been fairly made, till a ſecond trial evinced with certainty, that eighty-three pounds of the ſand would produce a barr of excellent iron weighing fifty pounds: a prodigious yield indeed, and far beyond what I have ever heard of from the richeſt common ores that are any where to be found; moſt of the ores I have ever met with or heard of, yield little more than half in pig metal, and which will ſuffer a waſte of near $\frac{1}{6}$ part to make tolerable good barr iron, and much more, if I am rightly informed, when the iron is intended for more valuable purpoſes, ſuch as being drawn into wire, &c.

After I had ſeen his addreſs in his letter to the Society, and his pamphlet; by the aſſiſtance of my friend Mr. Collinſon, I ſent him over two or three hints, which I judged might be of ſome ſervice to him; this produced the favour of a letter from him, of which the following is an exact copy.

## To Mr. Henry Horn.

Killingworth, Oct. 4, 1762.

Sir,

I Underſtand by Mr. Collinſon, that you have ſeen, and greatly approve of, the ſample of ſand-iron which was ſent; that you are deſirous to know how it was made, and whether it can be made in large barrs. The little barr you ſaw, was cut off from a barr of 52 pounds and a half, the firſt that was made at my ſon's work, the firſt that was ever made in America, and probably the firſt that was ever made in the world, in that manner, and ſo large a barr. I never heard of any attempt made upon the iron ſand, till that of yours 20 years ago, of which Mr. Collinſon gave me an account in his letter.

As to the manner of making the iron, it is wrought or ſmelted in a common bloomary, in the ſame manner as other iron ore is ſmelted; excepting this difference, this iron ſand is ſo pure, ſo clean waſhed, that there is not a ſufficient quantity of cinder or ſlagg to promote and perform the ſmelting, therefore we add either the ſlagg which iſſues from other iron, or elſe add ſome bog mine ore, which abounds with cinder; in this way it is as capable of being wrought as rock ore or bog mine.

I was in hopes that if this iron ſand could be wrought at all, the particles being ſo very fine, it would ſmelt very quick; but herein I found myſelf miſtaken, every particle has a will of its own, and muſt have its own particular ſmelting, for inſtead of its being performed in leſs time, it took more than common iron ore; but, upon farther experience, and more acquaintance with this ſand, the workman has ſhortened the operation from five hours down to three: if by any means it might be reduced to the ſame time with pigg iron, it would be a moſt uſeful improvement. If you can afford any directions to haſten the operation, I ſhould be greatly obliged for any inſtructions.

There is ſo much of this ſand in America, that I am apt to think, that there is more iron ore in this form of ſand than in mines.

I have written an eſſay upon the ſubject, which I hope Mr. Collinſon will let you ſee, as I hope to ſee what you are about to publiſh. My ſon has a ſteel furnace, which was erected ſeveral years before the act of parliament prohibiting them in the plantations: he has converted ſome of the ſand-iron into ſteel, of which I ſend you a ſample; as alſo a ſample of the iron. As my ſon had no inſtructions for making ſteel, we were forced to hammer out the ſkill by various trials as we could; ſo conclude that he is ſtill imperfect, and wants your help and direction to bring it to perfection, in which art I underſtand that you are a perfect maſter, and withal kind enough to offer your aſſiſtance; for which I am very thankful, and look upon it as an additional favour, if you will be pleaſed to indulge me with the benefit of your correſpondence, for I live in a corner of the world where ſuch information as, I truſt, you are able to furniſh, will be highly beneficial. Previous to my attempt of making iron from ſand, I propoſed my project to thoſe who were the moſt ſkilful in thoſe affairs, but met with nothing but what was diſcouraging; yet after all, had a perſuaſion of the practicability of the thing to a degree next to enthuſiaſm, ſo that I could not reſt till I had made trial. I am glad that the iron has ſuch qualities as to meet with your approbation; I knew that the iron was good, but did not know that it was ſo good as your ſuperior knowledge has found it. I want to know what ſuch iron will ſell for in England, whether it will be worth while to ſend it. This black ſand is a treaſure that has long lain hidden from the world, and is what may render the colonies more valuable to Great Britain.

I am, Sir,

Your moſt obliged

humble ſervant,

JARED ELIOT.

P. S. The barrs of iron which have hitherto been made of ſand, are from fifty to fifty groſs, hope in time to have them reach to ſeventy pounds weight each ; experience muſt determine that matter ; we can do better than at the time the eſſay was written. We have been viſited with a long and ſore drought, have done nothing for a long time for want of water.

The ſamples which accompanied this letter, were two ſmall barrs, weighing only a few ounces, one of the iron made from the ſand, the other of ſteel made from the ſame iron. Theſe barrs I have tried, and found that the barr of ſteel worked extreamly well under the hammer, was very pure and clean, and free from flaws. On the contrary the barr of iron turned out much otherwiſe, for, though it appeared to bear the force of the hammer, as well as the ſteel, yet it was not near ſo pure, but broke out in flaws and hollows, almoſt through the whole of the barr, and which a welding heat would by no means bring into proper union; this however engaged us to try a different method, which was, when the barr was reduced into a proper ſize for the purpoſe, to double it up three times, one part of the barr upon the other, and to try if it would then bear welding and become more conſiſtent, and by this means we found the end perfectly well anſwered; for it bore the force of the fire and the hammer, and became in a manner perfectly ſound. This ſevere trial proved, to a demonſtration, that the iron poſſeſt all that agreeable toughneſs and ductility, for which the Spaniſh iron is ſo deſervedly famous, without partaking of that vile red-ſear quality, for which the latter is very remarkable, and manifeſtly tends to prove the excellency of this ſand-iron, when reduced into bar-iron under proper care and circumſpection.

You will obſerve, Sir, from the letter, that this ſand is ſo pure, and ſo clean waſhed, that their firſt method of reducing the ſand to bar-iron proved too tedious, for want of ſome of thoſe adventitious materials, to promote and perform

form the ſmelting, and which always accompany the common ore, whether it be of the rock or bog kind; which materials, mixing with the matter, made uſe of by way of flux, and uniting with the aſhes of the fuel employed in melting down the ore, is uſually run into a thick opake glaſſy ſubſtance, forming, as it were, a covering over the metal, which, by its gravity, naturally ſinks to the bottom; this the workmen call cinder. Now the want of this matter rendering the operation too tedious, I find they had recourſe either to this cinder brought from other iron works, or to a quantity of the bogmine, which, I doubt not, would abundantly furniſh matter for cinder. If they had uſed only the firſt, and that properly choſen, it might very probably have been of ſome ſervice, without doing any material injury to the metal; but if the bog mine is uſed, though the ſervice might be apparently more, yet in all likelihood the injury would be infinitely great; and I am inclined to believe that ſomething of this kind occaſioned the difference obſerved between the two barrs above mentioned, viz. that the one might have been reduced by the help of more pure materials, and the other by the aſſiſtance of their bogmine, whoſe conſtituent parts abound with many impurities, ſome of which, by mixing with the metal, may have occaſioned the defects above complained of, and which required ſo ſevere an operation both of the fire and hammer to ſeparate from it. I am therefore of opinion, that as the proſecution of this uſeful diſcovery deſerves the greateſt encouragement, if the Society of arts and manufactures ſhould take it under their patronage, the premium they may think proper to propoſe ſhould rather be given to the perſon who ſhall produce the pureſt metal, than to him who ſhall produce the greateſt quantity; for otherwiſe, I am afraid, we ſhall be deprived of what I ſhould eſteem the moſt valuable part of this diſcovery; I mean the obtaining a more pure, and better kind of iron, than any we have hitherto been poſſeſt of, and which I am certain this ſand, under proper management, is capable of producing.

I am, Sir,

With the greateſt reſpect,

Your moſt obedient

humble ſervant,

Feb. 5, 1763.

HENRY HORNE.

# OBSERVATIONS
# ON THE
# NATURE
# OF
# IRON AND STEEL, &c.

# OBSERVATIONS

## On the NATURE of

## IRON AND STEEL, &c.

SINCE the method of refining ſteel, firſt converted in the common way from bar-iron, by remelting it in proper crucibles, and running it into ingots, has been known and practiſed here in England, where, for aught I can learn to the contrary, the diſcovery was firſt made; perſons of curioſity have appeared more than commonly deſirous of being made a little better acquainted with the nature of this uſeful proceſs. On which account, ſeveral very ingenious and worthy friends have warmly ſollicited me to proſecute a ſcheme of this nature. I could wiſh, that a perſon better qualified had been pitched upon for ſuch an undertaking; however in return for their candid opinion of my poor abilities, I think myſelf under a neceſſity of complying with their requeſt. And in the proſecution of ſuch a deſign, I ſhall review, and make the beſt uſe I am able, of a large number of papers, many of which, having been copied as notes upon the trial of a very conſiderable number of ſucceſſive experiments upon the ſubject of iron and ſteel, have long lain by me.

From this collection I flatter myſelf I ſhall be able to ſelect a number of hints, which (though not arranged perhaps with ſuch accuracy and preciſion as might be expected) may be of ſingular uſe in an affair of ſo important a nature.

I propoſe then in the firſt place to make a few obſervations relative to the nature of iron, conſidered in its mineral ſtate, as it is lodged in, or immediately taken from the bowels of the earth.

I ſhall next proceed to ſhew the methods which are commonly made uſe of to ſeparate the metallic atoms from the heterogeneous parts, with which the ore, when firſt taken from the mine, always more or leſs abounds.

I ſhall after this drop a few ſelected hints, on the neceſſary proceſs the workmen are obliged to purſue, in reducing it into bar-iron; which will always turn out a better or a worſe ſort of metal, according as more or leſs precaution has been uſed in the various ſtages of the operation.

We ſhall then be qualified to point out, what ſort of iron is the moſt proper for the purpoſe of being converted into good ſteel; with the nature of the proceſs by which that change is brought about.

In the laſt place, I apprehend, it may not be amiſs to give a brief account, both hiſtorical and phyſical, of the manner, as well as utility, of reducing the bar-ſteel to a more compact and uniform texture, by remelting it in a crucible, and running it into ingots; by which means, if the operation be well performed, and proper ſteel made uſe of, the metal becomes much better adapted to the purpoſe of making the fineſt edged inſtruments; as razors, lancets, &c. A very modern method this of procuring excellent ſteel for the above purpoſes; ſince no longer ago than the time when that very accurate writer Mr. Reaumur publiſhed his moſt elaborate treatiſe on converting bar-iron into ſteel, he as well as others, judged it abſolutely impracticable to melt bar-ſteel, without entirely deſtroying its malleability.

But to reſume the ſubject: in the proſecution of which, my propoſed method obliges me, in the firſt place, to take ſome notice of iron as a mineral.

Many and various are the accounts of this matter to be met with in our Engliſh authors; but moſt of thoſe accounts, which have fallen under my obſervation, are delivered in terms of darkneſs and obſcurity; indeed very ſhort hints only, with regard to iron as a mineral, are here and there to be picked up, with little or no connection. Mr. Chambers, in his Dictionary of Arts and Sciences, ſays: "Iron conſiſts of an earth, ſalt, and "ſulphur; but all impure, ill mixed and "digeſted, which renders it extreamly "liable to ruſt." He then proceeds to give ſome very imperfect hints, moſt of them trifling, picked up, I imagine, from various unſkilful workmen, who are in general wonderfully fond of noſtrums: but theſe, it is my opinion, do not merit any notice.

He next proceeds to lay before his readers an imperfect account of ſeveral kinds of iron, as he calls them; but without any real foundation in nature, only as they happen to come from different parts of Europe; from which circumſtance he not only preſumes to fix their names, but, very unjuſtly, to eſtabliſh their different characters; as fancy, or miſinformation, gave him the bias.

This matter I ſhould have paſſed over in ſilence, if I had not been in ſome meaſure anxious to preſerve ingenious and inquiſitive perſons from being impoſed on, for want of better information. Indeed as I proceed, I apprehend I may be obliged to recollect and examine ſome of Mr. Chambers's hints on this ſubject, in order to remove ſome miſtakes, which may be judged neceſſary to be removed.

From the General Dictionary of Arts and Sciences, publiſhed about twenty years ſince by Mr. Barrow, a few particulars more may be collected. This author obſerves, that "true native iron is "not to be expected in the midſt of "maſſes of its ore, but in detached ſprigs "or filaments, in the fiſſures of rocks, "the whole ſubſtance of which is rich "in that metal: ſuch have been all the "genuine ſpecimens of this rich foſſil, "and ſuch their place of formation." He goes on: "The ores of iron generally diſcover themſelves to be rich, "either by their reſemblance to wrought "iron in ſtructure and look, or by the "yellowiſh or purpliſh tinge they are

" coloured with. Thoſe which have " moſt of all the appearance of the metal " they contain, are uſually the richeſt." More hints follow under the ſame article, which the reader may conſult at his leiſure.

But the moſt accurate account I have met with upon this ſubject, taken from fact and experience, is in a treatiſe upon mining, written at the requeſt of the late Duke of Chandois, by one *, who, tho' the learned differ as to the principles of his philoſophy, was allowed on all hands to underſtand the buſineſs of mining as well as, if not better than, any man in England. " Iron," (ſays he), " is found " in thin ſtrata of ſtone, in nodules

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* The late John Hutchinſon, Eſq; in the 12th vol. of his works, p. 187, 188.

" lodged ſome in, ſome between the " ſtrata, in veins, formed in ribs and " ſhoots, in bellies, ſome in form of " ore, ſome like clay, and ſome by its " capacity attracted into the moſt regu- " lar and beautiful figures, formed, inde- " pendent, round from a center, ſtriated, " and compoſed of infinitely ſmall ſpheres, " divided into ſmall cones, ſome of them " having attracted new maſſes, and " formed new centers and arches over " them in like order; it is ſeldom found " native; I have found it very pure, but " never malleable, or flexible, nor can " fuſion in fire ever make it ſo, I think *, " becauſe

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* This ingenious writer happens in this remark to be a little miſtaken; for though it is true, that iron is not to be found malleable in its mineral ſtate; as the vaſt quantity of foreign matter, which frequently amounts to one half of the compound, keeps

" becauſe it is attracted into maſſes; it " is generally found red, or of a ruſt " colour; in ſome places it is found " black, grey, white, &c. It is generally " very heavy, and ſome of it very hard." Thus far this ingenious writer.

Poſſibly it may be imagined, that enough has been ſaid upon the nature of

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keeps the metallic atoms ſo far aſunder as to render it impoſſible to determine, whether, could they be brought into contact without fuſion, they would be malleable or not; yet, that fuſion cannot render it malleable, becauſe it is attracted into maſſes, when run down in the large furnace, does not prove it abſolutely impoſſible to reduce it by fuſion into ſuch a form as to inſure its malleability. I have myſelf more than once, in ſmall quantities, run it down in ſuch a manner, that inſtead of being attracted (as this writer obſerves) into maſſes, it has aſſumed a fine grainy appearance; a certain ſymptom of malleability: but then it has always turned out, not malleable iron, but excellent ſteel.

this metal, conſidered in its mineral ſtate; and that we ſhould now proceed to give ſome account of the means neceſſary to free the metallic atoms from the incumbering heterogeneous matters, with which, as taken out of the earth, they are ſo very ſtubbornly connected. But before I go on to that part of my ſubject, I think it highly requiſite to remove a difficulty, which moſt of our Engliſh writers upon iron have left as an obſtacle in our way.

I took notice in page 41, that Mr. Chambers, in his Dictionary, hath very arbitrarily aſſigned names and characters to different ſorts of iron, according to the different countries where they are produced; this he has done in ſuch a manner, (though without any real judgement),

ment), as to give the world a very high opinion of the iron of one country, to the great diſparagement of that produced in another.

"There are (ſays he) ſeveral kinds of "iron, which have properties very different from one another. As Engliſh "iron, which is coarſe, hard, and brittle, "fit for fire-bars, and ſuch uſes. Swediſh "iron, which is a fine, tough ſort, will "beſt endure the hammer, is ſofteſt to "file, and in all reſpects the beſt to work "upon. Spaniſh iron, which would be as "good as the Swediſh, were it not ſub"ject to be red-ſear. German iron, com"monly called among us Dort Square; "this is a coarſe ſort of iron, and is "only fit for ordinary uſes." But here comes the cream of the jeſt, (if I may be indulged the expreſſion,) in the following miſtake in fact, and contradiction to what he had before ſaid. " There is," ſays he, " another ſort uſed for making wire, " which is the ſofteſt and tougheſt of all: " This is not peculiar to any country; " but is indifferently made, wherever " iron is made, though of the worſt ſort; " for it is the firſt iron that runs from " the mine-ſtone, when it is melting, " and is reſerved purely for this purpoſe " of making wire. * "

From ſuch miſtaken notions of the ſuperior value of the iron of one country

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* Iron wire is, and certainly muſt be, drawn from good metal, that is from iron thoroughly cleanſed and well purged; but it makes no difference, as far as I can find, whether it be from the firſt, ſecond, or laſt running.

to that of another, it has ariſen, that the Swediſh iron has had the preference given it for making of ſteel; and while we have ſhewn a fondneſs for particular ſorts of it, at almoſt any price, we have neglected to improve the materials to be found both in our own country, and in our colonies. In my examination before the Houſe of Commons, while the American iron bill was depending, I aſſerted that this undue preference of Swediſh to other iron, was a mere vulgar error; I repeated it upon oath before the Houſe of Lords; and I now again declare it to be an abſolute fact, that the iron of any country, if properly prepared, is equal to the Swediſh for being drawn into wire, or making ſteel; for I have made from our Engliſh iron, ſteel not at all inferior, if not ſuperior, to any that was ever made from the beſt Swediſh ; and certain cutlers have chearfully given me for it four times the price at which they could have purchaſed the beſt bliſtered ſteel, made from Swediſh iron : A ſmall quantity of this ſteel I have ſtill by me.

Thus it appears, that the iron of all countries indifferently is fit for the making both of ſteel, and of wire; not for the reaſon Mr. Chambers aſſigns in this latter caſe, namely, becauſe it is the firſt running from the iron-mine ; but becauſe it is refined to a higher degree, than is neceſſary for common and ordinary purpoſes ; and when once iron is ſufficiently purged to be drawn into wire, or formed into tin-plates, it then becomes extreamly well adapted to be converted into ſteel. The importance of this digreſſion will, I hope, be judged a ſufficient apology for its length.

I am now to ſhew the methods commonly made uſe of to ſeparate the metallic atoms from the foreign matter, with which they are united in their mineral ſtate.

I have obſerved before, p. 40, that Mr. Chambers in his Dictionary, takes notice that iron in its mineral ſtate conſiſts only of an earth, ſalt, and ſulphur impurely mixed.

The very ingenious Mr. Reaumur, in the beginning of his treatiſe upon the art of converting bar-iron into ſteel, gives a much more accurate account of this matter; where he informs his readers, that the ores of iron are compoſed of ferruginous parts, earthy parts, ſulphureous and ſaline parts. A more accurate account, I think, cannot be given of the component parts of iron in its mineral ſtate. "Art," he obſerves, "has found out "the means to ſeparate the metallic or "ferruginous parts from the foreign "ones, with which they are mixed, and "to reunite and form them into maſſes, "which render them proper for different "purpoſes; to arrive at which," ſays he, "fuſion is the firſt means to be uſed." But he ſhould have remarked, that previous to fuſion it is abſolutely neceſſary to roaſt the ore. Dr. Harris, in his Lexicon Technicum, has given under the article of Iron, tranſcribed from the Philoſophical Tranſactions, No. 137, or from the end of a little treatiſe, entitled, Mr. Ray's Collection of old Engliſh Words, the preciſe method, how this operation is performed at the iron works in the foreſt of Dean in Glouceſterſhire. This is done, he obſerves, without fuſion of the metal, and ſerves to conſume the droſſy part of the ore, and to make it friable. The Doctor here gives a very exact account of the operation; but does not, I apprehend, completely aſſign the reaſon, which renders it ſo abſolutely neceſſary; which is, not only to make the ore friable, but alſo to ſet at liberty, and carry off a ſufficient quantity of that ſtubborn, binding ſulphur, which is originally inherent in the ore; and thereby render it more diſpoſed to give up with greater freedom, and conſequently in greater abundance, its more valuable contents. Without this operation being made previous to the fuſion, I am ſtrongly inclined to believe, from facts I have frequently obſerved, that the whole compound would, by the violence of the fire, run down together into one vitrified maſs, from which the metallic parts could ſcarcely ever, if at all, be ſeparated; at leaſt, not without ſo much labour and expence as would render it hardly worth while to attempt it. The Doctor then goes on to give a very particular account of the ſmelting furnace, I preſume, taken from the ſame Tranſaction: to which I refer the reader, to conſult it at his leiſure.

I have viſited ſeveral of theſe ſmelting furnaces in different parts of the kingdom, particularly in Staffordſhire, Yorkſhire, and Suſſex. The forms of all I have ſeen have been pretty much alike, and their methods of working nearly the ſame. Notwithſtanding which, I have met with very different ſorts of iron at different places. This, I allow, might in ſome meaſure be owing to their working upon different ſorts of ore; but upon the moſt ſcrupulous obſervation I could make, I cannot be of opinion, for ſeveral reaſons, that this could be always the caſe; for I frequently obſerved, almoſt at every furnace where I have been, that they worked upon ore ſo ſimilar in every reſpect to that I met with at other furnaces, (and I made the niceſt obſervation and the moſt particular enquiry I could), that I began ſtrongly to ſuſpect ſome latent cauſe, not obvious to general notice, muſt lie at the bottom of this myſterious affair.

This conſideration naturally led me to enquire what ſort of flux was made uſe of at different furnaces, and whether the difference of the iron produced might not, in great meaſure, be owing to the application of different fluxes. I was pretty ſoon convinced that my ſuſpicion was very far from being groundleſs. In Staffordſhire, and ſome parts of Yorkſhire, I found that their iron was in ſome places extreamly bad, and in others but very indifferent; whereas at ſeveral of the furnaces in Suſſex, I was well aſſured, their iron turned out much ſuperior in quality to what was to be met with in the before mentioned counties. Upon farther enquiry, I became fully ſatisfied, that the different materials uſed as a flux, had no ſmall ſhare in the difference of the iron produced in theſe different parts of the kingdom.

I ſhall venture to give the reaſons of my opinion upon this intereſting part of my ſubject.

In Staffordſhire, and various parts of Yorkſhire, I found the matter made uſe of by way of flux, was generally unburnt lime-ſtone; a ſubſtance I can by no means think proper for the purpoſe. Iron, it is well known, is one of the moſt powerful abſorbers of impure acid ſulphur in nature; this is ſufficiently manifeſt from its being made uſe of as a flux for the trying of ſamples of lead-ore, as well as for the precipitating the martial regulus of antimony. In theſe caſes, it makes a very ſtrong, uſeful and purifying flux; as it very eagerly imbibes the impure ſulphur, ever to be met with in the lead-ore and antimony in great abundance, ſoon becoming intimately united, and riſing with it to the ſurface, where it forms a very impure ſcoria; while at the ſame time, in one caſe, it leaves the lead pure at the bottom of the crucible; and in the other, a pure martial regulus of antimony.

Were we to take it for granted, that the lime-ſtone, uſed as a flux for reducing and collecting the metallic atoms with which the iron-ſtone abound, acted in the ſame manner as in the caſes above mentioned, it might perhaps be ſafely made uſe of. But I am well ſatisfied, by a great number of experiments which I have made with the utmoſt caution and preciſion, that it acts in a manner directly oppoſite; for here the earthy part only of the lime-ſtone mixes with the earthy and ſaline parts of the charcoal, employed in the operation; which being much lighter than the metalline parts of the ſtone, riſes and ſwims upon the ſurface of the metal, where it forms an inert, inoffenſive, glaſſy ſubſtance; while the noxious, poiſonous matter, contained in the lime-ſtone, being ſtrongly attracted by the iron, unites and ſinks with it to the bottom, and there forms a rotten, red-ſear, bad iron *.

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* In a ſmall pamphlet upon ſpar, lately publiſhed by the ingenious Dr. Hill, he gives an account of the generation of a fluid ſulphur in lime-rocks, in the following manner:

" The primitives of ſpar, as we have ſeen, are, water, bitumen, chalk, clay, talc and mineral acid: to theſe the operations of the air, and fire, give great powers of acting. We thus find heavy vapours, formed of air, and much water. Theſe pervading all things, meet the mineral acid, and uniting with it; if they run clear to the ſurface, afford

This I may have occaſion to mention again, by way of proof, as I proceed.

I ſhall now with freedom give my opinion of different ſubſtances, applied as fluxes for reducing iron-ore, at other places; and of the propriety with which they are uſed for that purpoſe.

In an iron work, at a place called Roberts-bridge, near Winchelſea, and in

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afford medicinal ſprings; but thus united, they may fall upon bitumen. This is no where more frequent than in lime-ſtone rocks; and often ſtands in puddles, in their natural hollows. By this mixture, uniting in its courſe, is formed a real, though a fluid ſulphur: for ſulphur is nothing elſe, nor can it be formed by any other means. This ſulphur, not yet concreted, paſſes in its liquid form, through the pores of the lime-ſtone; diſſolving part of its purer chalk as it goes."

If Dr. Hill be right, as I apprehend he is, in the preceding obſervations, what I have ſaid above, muſt appear to be the true ſtate of the caſe.

another within a few miles of Tunbridge-wells, both in Suſſex; they make uſe of a certain foſſil, to which they give the name of Greys. Why this foſſil is ſo called, I muſt leave others to determine. The materials, of which it is compoſed, are a congeries of different ſhells of fiſhes, ſlightly bound together by a ſort of reddiſh earth, not very unlike ſome kinds of iron-ore. Large ſtrata of this foſſil are lodged in the earth, not far diſtant from theſe two iron works. And it is very ſafely, and to great advantage, made uſe of as a kindly abſorbent flux; which being run down with the ore, inſtead of adminiſtering any bad quality to the metal, deprives it, in a great meaſure, of that noxious, arſenical ſulphur, which too often abounds in its compoſition. And it may be proper to obſerve, that the iron produced at theſe two different works, is always a fine, genuine, good ſort of metal; poſſeſſed of almoſt every good quality, that can be deſired.

I ſhall mention but one ſort of iron more, with regard to fluxes; namely, that which is wrought at the foreſt of Dean. This iron, I believe, has been celebrated by almoſt every writer upon the ſubject, of any conſideration. I beg leave juſt to recite their ſeveral opinions; and to tranſcribe an entire paragraph from the ingenious Dr. Harris, in his Lexicon Technicum; a work, in my humble opinion, not excelled, by any of his followers in that way of writing, unleſs by the mere addition of a number of articles, which, at the time when he wrote, had not been ſo particularly conſidered.

" IRON. In the foreſt of Dean, in
" Glouceſterſhire, the beſt iron-ore is of
" a bluiſh colour, and is called Bruſh-
" ore; but this being melted alone, pro-
" duces a metal very ſhort and brittle:
" to remedy which inconvenience, they
" make uſe of cinder, which is found in
" great quantity where any old works
" have been in that country: for in
" former times, their bellows being moved
" only by hand, their furnaces produced
" a fire much leſs intenſe than thoſe they
" now employ: ſo that formerly they
" melted down only the principal part
" of the ore, rejecting the reſt as uſeleſs.
" This refuſe is the cinder; which being
" mingled with the ore in a due quan-
" tity, gives that excellent temper of
" toughneſs, for which this iron is
" preferred before any brought from
" abroad."

Mr. Chambers, before taken notice of, and, I would hope, not unjuſtly cenſured, in his Cyclopædia, expreſſes himſelf thus:

" We have a great number of iron
" works in moſt parts of England; thoſe
" in the foreſt of Dean are in moſt re-
" pute. The ore is found there in great
" abundance, differing much in colour,
" weight and goodneſs. The beſt, called
" Bruſh-ore, is of a bluiſh colour, very
" ponderous, and full of little ſhining
" ſpecks, like grains of ſilver; this af-
" fords the greateſt quantity of iron,
" but being melted alone, produces a
" metal very ſhort and brittle, and there-
" fore not ſo fit for common uſe. For
" remedying whereof, the workmen make
" uſe of another ſort of material, termed

" cinder, which is nothing but the re- " fuſe of the ore, after the metal has " been extracted; and which being " mingled with the other, &c. cauſes it " to be preferred to any brought from " foreign parts."

This cinder may be conſidered in a twofold capacity; for, as Dr. Harris obſerves, the Danes making uſe only of hand-bellows, which producing, as he remarks, a much leſs intenſe fire, than is raiſed by the bellows now made uſe of; a conſiderable quantity of the metal muſt be left in the cinder, which being again run down with freſh ore, muſt of courſe be given up; while the other part of the cinder, may probably yield a very ſalutary and uſeful flux; for we hear of no other flux being there made uſe of.

Barrow, in his Univerſal Dictionary, takes but very little notice of this excellent Engliſh iron, comparing it only with ſome iron-ore, found in Hartz-foreſt, in Germany; which he ſays, is the richeſt iron-ore he knows of, except ſome of the Hæmatites; but adds, that we have ſome very like it in the foreſt of Dean, which is at preſent worked to great advantage.

The very ingenious and inquiſitive Dr. Shaw, in his notes upon Boerhaave's Chemiſtry, p. 95, ſeems to have copied from Dr. Harris, or rather, perhaps, from the Philoſophical Tranſactions, No. 137, from whence, as before obſerved, Dr. Harris took his hints: and he alſo allows, that the foreſt of Dean iron is preferable to any other.

I have dwelt the longer upon this article of fluxes, as I think it a matter of the laſt importance; ſince this being well underſtood, I am fully of opinion, that we have not the leaſt occaſion to be ſollicitous, with regard to any iron imported from abroad, unleſs from our own colonies; as from thence, or from our own ore here at home, we may be furniſhed with any ſort of iron we can poſſibly want.

It fully appears from the almoſt univerſal ſuffrage of our Engliſh writers, that from the foreſt of Dean we may be ſupplied with iron, ſome ſay as good, others better than any that comes from abroad.

But poſſibly it may be objected,— "Taking this for granted, what light have we from thence, relative to the doctrine of fluxes, (a ſubject you have ſo long dwelt upon), when it does not appear, that among all that you have collected concerning the foreſt of Dean iron, the word flux is ſo much as once mentioned * ?" It is very true, the word is not mentioned in the printed accounts I have cited concerning this ſuperexcellent iron. But we are very amply informed, that the bruſh-ore of the place is not inferior to the ore of any country in the world; that this, melted alone, would turn out very ſhort and brittle; but being mixed with the old cinder in a due proportion, it furniſhes us with better bar-iron, than any that comes from abroad.

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* In a paragraph added p. 65, ſince I wrote this part, the word is mentioned, and, I apprehend, with propriety.

In every account we meet with of this famous bruſh-ore in the foreſt of Dean, not a ſingle hint ever occurs, of the uſe of any other flux than the cinder; and I will venture to affirm, as I know it, from my own experience, to be true, that let a ſufficient quantity of this cinder be tranſported to the iron-works in any other part of the kingdom, and made uſe of inſtead of the lime-ſtone; the iron produced will be nearly, if not equally, as good as that of the foreſt of Dean.

If the doctrine of fluxes be not implied in this narrative, I know not where to find it; and if our iron-maſters, as they are called, are not capable of drawing proper inferences from ſo plain an account, I am very certain it muſt be their fault, not mine.

I ſhould now, in courſe, give a few hints relative to the methods employed at the forges *, for reducing the ſow, or pig-metal, there melted down, into bar-iron; but I muſt firſt mention one intereſting particular, namely, the abſolute neceſſity there is, in order to make good bar-iron, that the flux uſed in the furnace, be of ſuch a nature and conſiſtence, as to render it apt and proper to take up as much as poſſible of the common earth, which greatly abounds in the mine-ſtone, when firſt committed to the furnace. If this be left to be done at the forge, called the Finery, it muſt either occaſion a great

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* Theſe forges are two, which are denominated the Finery, and the Chafery. They are deſcribed by Dr. Harris in his Lexicon Technicum, and in the Philoſophical Tranſaction before referred to, No. 137.

waſte of metal, or the bar-iron will turn out very bad and rotten.

Having premiſed this, I ſhall endeavour to give ſome account of the methods uſed at the forge, called the Finery.

This forge is very properly ſo named, becauſe here the ſow, or pig-metal, is to be freed as much as poſſible from thoſe extraneous matters, which in part conſtitute the ore when committed to the ſmelting furnace; and alſo from an undue proportion of ſuch matter as it may acquire in its paſſage through the ſmelting furnace, either from the flux, or the fuel made uſe of there.

The operations at this forge are well deſcribed in the before quoted Dr. Harris, under the article of Iron, or in the Philoſophical Tranſactions, No. 137; to either, or both of which I again refer the reader for his information; confining myſelf merely to point out the conſequence of ſuch operations, they having a tendency to furniſh us with a better or worſe ſort of iron, as they are differently managed.

As this forge then, called the Finery, furniſhes us with very different kinds of iron; owing either to the different ſorts of ore made uſe of, to the different manner of fluxing the ore, or to the difference in the care and caution of the workmen at the Finery; it may not be amiſs to take notice of the different ſorts of bar-iron, to be met with in the common markets for this commodity, and the ſeveral denominations under which the workmen have ranged them.

In general then, theſe different ſorts of bar-iron bear the following different names, as deſcriptive of their different characters, viz. Red-ſear, Cold-ſear, and Tough.

*Red-ſear* is of ſuch a quality, as to yield, in a certain degree, to the hammer when it is cold; but is apt to fly to pieces under its impreſſions at a low-red heat, and becomes brittle, when between hot and cold.

*Cold-ſear* is of ſuch a nature, as to be very brittle when it is cold; but it eaſily bears the ſtrokes of the hammer when it is hot, almoſt at any degree of heat, *Tough* is of ſuch a nature, as to be very flexible when cold, and capable of ſuſtaining pretty harſh treatment from the hammer, either hot or cold.

But among theſe different kinds of bar-iron, are to be found different ſpecies of each ſort: ſome partaking more or leſs of the red-ſear quality, ſome more or leſs of the cold-ſear, and ſome more or leſs of the tough than others.

As the difference of theſe kinds of bar-iron muſt ariſe from different cauſes, it may not, I apprehend, be amiſs to ſpend ſome little time upon an enquiry into theſe cauſes; in order to which, it may be proper to examine what different ſorts of matter enter into the compoſition of what we call iron-ore, or mine-ſtone.

Mr. Reaumur, as before hinted, has laid a proper foundation for a full and ſatisfactory anſwer to this enquiry, when he informs us, that iron-ore is compounded of two ſorts of earth, a metallic, and a common one, ſtrongly united by ſalt and ſulphur. And I am humbly of opinion, that the ſalt gives this metal its permanency, while the ſulphur occaſions its very ſtubborn coheſion. It has been before obſerved, p. 52,—54, that the ſulphur coheres to, and binds the different parts of the compoſition together in ſo obſtinate a manner, that if a conſiderable part of it were not diſlodged and thrown off by a previous roaſting, it would be extreamly difficult, if at all poſſible, to procure a proper ſeparation.

Thus much being premiſed, I hope, I ſhall now be able to give a tolerable account of the different kinds of bar-iron.

The four different ſorts of matter abovementioned entering into, and making a part of, the compoſition of all iron-ores, and ſtill continuing their connexion, in different proportions, not only when they are run into ſow, or pig-metal, but even after they are wrought into bars; it muſt undoubtedly be owing to a more orderly proportion of theſe ſeveral materials, when the bar-iron proves tough and good; and to an undue and irregular exceſs of one, or the other of them, when the metal turns out bad, whether it be of too red-ſear, or too cold-ſear a quality: That this is really the caſe, will admit in ſome meaſure even of ocular proof. For inſtance: If the common earth abounds in too great a proportion, (which by the way is not eaſily ſeparable in the uſual methods of working), I ſay, if this exceeds its due proportion, it will infallibly produce a red-ſear iron, and the pig-iron of this ſort is eaſily known by its complexion, as it ever exhibits to the eye a very dark grey colour, ſometimes approaching almoſt to a black. The reaſon why this ſort of pig-iron ſhould produce a red-ſear iron, when wrought into bars, is extreamly obvious; as well as why it ſhould diſcover ſome degree both of ſoftneſs and toughneſs, when it is cold; for the common earth being too plentifully mixed with the other ingredients, and being in its own nature ſofter than the metallic atoms, renders the metal thus compounded, more liable to yield either to the ſtroke of the hammer, or the teeth of the file, while it remains cold: But when the more active parts of the compoſition are put into motion by the force of the fire, the earthy particles, being more ſubject to dilate and expand themſelves, than the more compact metallic atoms; the latter are eaſily driven to a greater diſtance from one another, by the expanſion of the former; and in this ſtate, I mean while it continues hot, almoſt the leaſt impreſſion of the hammer will make the parts ſeparate and fly aſunder; and thus it comes very properly under the denomination of red-ſear iron, whether it be in pig, or in bar. On the other hand, ſuppoſing the common earthy particles to be carefully ſeparated, but ſtill an undue quantity of the ſaline and ſulphureous to be left behind; theſe likewiſe may keep the metallic atoms too much aſunder, though not at ſo great a diſtance from each other, as when they are too much clogged with common earth; the conſequence in this caſe will not be the ſame as in the former; but on the contrary, the iron will now be very brittle when cold; but when heated, and that to almoſt any degree, will ſuſtain the moſt violent efforts of the hammer. The appearance put on by the metal in this caſe, whether in pig, or in bar, is that of large brilliant and ſhining maſſes, not altogether unlike thoſe diſcoverable in regulus of antimony when broken. And here likewiſe the enquiry is not attended with any great difficulty, why the bar-iron ariſing from this ſort of metal ſhould be cold-ſear, as it is called; that is, why the bars ſhould be very brittle when cold, as well as why they ſhould be capable of bearing a pretty high degree of heat, and, in that ſtate, of ſuffering without injury ſo much violence from the hammer. For as the ſaline and ſulphureous particles, being mixed in too large a proportion among the metallic atoms, naturally, and indeed unavoidably, throw the whole compoſition into pretty large ſhining maſſes, which comparatively ſuffer thoſe atoms to touch each other but ſparingly, and in a few points only; the metal, when cold, muſt of neceſſity be very fragile and brittle; but on the other hand, upon a proper application of the fire, as theſe ſaline and ſulphureous particles are eaſily reduced to a ſtate of fuſion, and as they may be conſidered as a ſort of flux to the metallic atoms, theſe atoms in this ſtate are brought to be ſoft and yielding, and eaſily ſubmit to the im-

impreſſions of the hammer, without ſuffering any material injury; and this ſort of iron, if worked with care, is of great uſe for many valuable purpoſes. If the ſalt and ſulphur be ſtill farther diſcharged, but yet ſo as that the metal remains cold-ſear in a lower degree, it will begin to diſcover a whitiſh, grainy texture, ſomewhat reſembling the grain of ſteel when broken, after its pores have been cloſed by the hammer; and in this ſtate it is very properly called by the workmen, cold-ſear tough. This grainy appearance is evidently owing to the metallic atoms being brought near together, as the ſtill abounding quantity of the other extraneous matter is driven off by the fire, and preſſed out by the force of the hammer. This ſort of iron is juſtly preferred by the workmen to the former, as it is better adapted to many more purpoſes, and thoſe too of greater uſe. Thus, by ſtill going on to diſcharge yet more of the ſalt and ſulphur, the appearance of the iron to the eye goes on ſtill to vary its texture; for in proportion as this diſcharge is made, the grains are brought ſtill into a cloſer contact, and again begin to form themſelves into larger coalitions; but of a quite different nature from thoſe which appeared in its cold-ſear ſtate; for they now begin to diſpoſe themſelves into longiſh fibres, with perhaps here and there grains ſtill interſperſed; till at length, by ſtill carrying on the operation, the metal is entirely formed into a fibrous texture; and theſe fibres become ſtill larger and longer, as the other principles are more and more diſcharged: and in this ſtate the iron becomes duly qualified for all the more valuable purpoſes, eſpecially for the moſt valuable of all, that of being converted into the moſt excellent ſteel *. Here it might not be improper to offer a caution againſt going too far in this proceſs of refining the iron (though I believe our iron-maſters, as they are called, are in very little danger of this, as it would be attended with a loſs of metal, and conſequently with a diminution of their profit) ſince by carrying this operation too far, the iron may again be reduced, though upon a quite different principle, to ſuch a ſtate as will render it extreamly brittle, if not quite rotten and altogether

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* If any of my readers are deſirous to ſee a fuller exemplification of theſe different appearances, in the texture of the bars of iron, under the different ſtages of purification, I would recommend to them Mr. Reaumur's fifth Memoire, together with the plate annexed, No. 6.

uſeleſs, either hot or cold; for, a certain proportion of ſulphur being, as before obſerved, abſolutely neceſſary to maintain the metallic atoms in a ſtate of union and coheſion, hence, if this vinculum or binding principle be carried off, the iron muſt again loſe its toughneſs and malleability, and become liable to fall to pieces under a very ſlight impreſſion.

In order to prove, that this would really be the caſe, I ſhall give a ſhort recital of an experiment, which I had the honour to exhibit ſome years ſince, before the late Martin Foulks, Eſq; the then worthy Preſident, and ſeveral other reſpectable members of the Royal Society. I ordered a round ball of iron, weighing about four or five pounds, to be fixed for convenience at the end of a long bar of the ſame metal. The ball was put into a large fire at one forge, (for there is need of two to make the experiment;) it was there heated to ſo great a degree, that it was almoſt ready to melt, and indeed till the ſulphur, (or cinder as the workmen chooſe to call it), was abſolutely in a ſtate of fuſion. Then being removed to another forge, where there was no fire, and there being dexterouſly applied by one man to the noſe of a large pair of bellows, and another man being placed to blow the bellows, and force the condenſed air with all his might into the pores of the heated ball; in a very few ſeconds the heat of the ball was increaſed to ſuch a degree of intenſeneſs, as to make the iron drop like melted wax; but ſo deprived of its ſulphur, which was thus by the violence of the blaſt ſublimed and carried off, that the melted metal left behind in a veſſel placed underneath to receive it, was become a perfect calx, or crocus of iron. In this manner I have ſometimes run off from the ball, four or five ounces of the metal thus reduced to a crocus; in which ſtate, if veſſels could be procured that would hold, no doubt it would vitrify in a very violent fire, and evince that a very fixed ſalt ſtill remained as part of the compoſition, and that the other part was a pure metallic earth; for I am well aſſured, that by introducing the other principle of ſulphur from any proper oleaginous matter, it may again be brought back to the ſtate of malleable iron, or ſteel, at the will of the operator; and in all probability the reſult would be, that the metal thus formed, would turn out the beſt iron or ſteel, that could be procured; as, by this means, it muſt become totally freed from any mixture or remains of the common earth, the great bane of both iron and ſteel.

As a juſt inference from this experiment and its conſequences, I cannot leave this part of my ſubject, without afreſh inculcating the abſolute neceſſity, in order to make good bar-iron, that the utmoſt care ſhould be taken to extract the particles of common earth, which all iron-ores are ſo exceſſively fraught with; and this ſhould be effected as much as poſſible at the ſmelting furnace, by the application of a proper conſiſtent flux: for if it is not done there, it muſt be done at the finery, where I am certain it cannot be done without a very conſiderable expence of the pure metal.

In order to confirm the juſtneſs of this obſervation, I ſhall tranſcribe a ſhort paragraph from the ingenious Mr. Reaumur, p. 391. upon the art of ſoftening caſt-iron, which is as follows:

*Les fontes blanches ſont plus pures que les fontes griſes, elles contiennent plus de fer; nous l'avons déjà vû; & nous en donnerons encore une preuve, qui eſt que dans les forges, on retire plus de fer forgé d'un certain poids de fonte blanche, que du même poids de fonte griſe. Il y a plus de matiéres étrangéres dans les fontes griſes, & ſurtout; probablement plus de matiere terreuſe, plus de matiere vitrifiée de ce qu'on appelle, dans les fourneaux à mine de fer, du Laitier.* "Pig-iron of a white colour is more pure than that of a grey; it contains more iron; we have ſeen this already, and we ſhall give yet another proof of it, which is, that at the forge, they extract more forged iron from a certain weight of white pig, than from the ſame weight of grey. There is a greater quantity of heterogeneous matter in the grey pig, and eſpecially, in all probability, more of the earthy, and more of the vitrified matter, of that which they call at the furnaces for iron-ore, Laitier."

As the affair of managing the metal at the forges, is the principal part worth notice, there is room to ſay but little as to the manner of their working there; only it may be proper juſt to take notice in general, that to the finery, which is the forge of which we have been treating, they bring, as Dr. Harris obſerves, their ſows and pigs, in order to refine them;

which they endeavour to effect in the following manner:

"Into the finery they firſt put the pigs of iron, placing three or four of them together behind the fire, with a little of one end thruſt into it; where ſoftening by degrees, they ſtir and work them with long bars of iron, and expoſe at different times different parts to the blaſt of the bellows, in order to refine it as equally as poſſible; till the metal runs together into a round maſs or lump, which they call an Half-Bloom. This they take out, and give it a few ſtrokes with their ſledges; they then carry it to a great weighty hammer, raiſed by the motion of a water-wheel; where applying it dexterouſly to the blows, they preſently beat it out into a thick ſhort ſquare.

This they put into the finery again; and and heating it red-hot, they work it out under the ſame hammer, till it comes to be in the ſhape of a bar in the middle, but with two ſquare knobs on the ends. This they call an Ancony. Which finiſhes the buſineſs at this forge.

"Laſt of all it is carried to the other forge called the Chafery, where they adminiſter other heats and more workings under the hammer, till they have brought their iron into bars of ſeveral ſhapes and ſizes."

Having thus far diſcuſſed and ſettled every thing which ſeemed to be neceſſary upon the article of iron, and clearly ſhewn what ſort of bar-iron is beſt for the purpoſe of being converted into good ſteel;

my method now requires, that I ſhould offer ſome thoughts upon the nature of ſteel.

And here I ſhall endeavour to diſcover its true characteriſticks, ſhew in what it differs from, and in what it agrees, at leaſt with ſome common bar-iron.

Mr. Chambers's account of iron I have already, I hope, not without reaſon exploded: but his account of the nature of ſteel, and the manner of its being prepared, or converted, as it is uſually ſtiled, from bar iron, deſerves, I think, for its obſcurity, a more ſevere animadverſion; unleſs we imagine he had been purpoſely obſcure, judging the ſubject to be of ſuch a nature as it would be criminal to divulge. The ingenious Dr. Harris, in his

Lexicon Technicum, has indeed been more explicit; and what he there offers upon the article of ſteel, is not only true, but practicable; ſo far as he enters into the ſubject. Beſides theſe two, I cannot recollect, that I have met with any Engliſh writer, who has offered any thing upon this matter worthy of notice. But it may poſſibly be expected I ſhould pay ſome regard to a very celebrated French writer, a late worthy and ornamental member of the Royal Academy of Sciences at Paris, to whoſe memory I ſhould be unjuſt, were I not to acknowledge myſelf greatly indebted to his elaborate performance upon the ſubject I have ventured to treat of in this imperfect eſſay: while at the ſame time I cannot help expreſſing my concern, that I am under a neceſſity of differing from him in that part of it, which I am juſt entering upon. But my diſſent even from ſo great a philoſopher will be excuſed, if I am able to give ſatisfactory reaſons for it, founded upon fact and experience. To begin then with iron, in the firſt ſtate in which it may not improperly be entitled to the denomination of ſteel; viz. when it is firſt reduced from its mineral ſtate into what we call pig-metal.

Mr. Reaumur, in the beginning of his 7th Memoire, very juſtly complains, that in the language of common writers upon the ſubject, there is no ſuch thing to be met with as any juſt idea of the nature of ſteel, and wherein it eſſentially differs from iron; that in general the definitions they have recourſe to, are iron more pure, iron more perfect, iron more refined. He names the great Rohault, and adds, that he could mention an hundred others, whoſe accounts are not more deciſive and accurate; upon which he very pertinently remarks; that with regard to the laſt mentioned definition, that it is iron more refined, it is indeed very faulty; nor will it at all hold good, if we compare it with the idea we have of refining gold, or ſilver, by which we intend the removal of impure metals, or any other heterogeneous matter which may happen to be incorporated with the gold, or ſilver. He goes on to make ſeveral other very ingenious remarks, well worth our attention; but which we have no occaſion to adduce, in order to prove that iron thoroughly purged and refined is by no means entitled to the character of ſteel; ſince we have before us ſo flagrant an inſtance to the contrary, as that pig-metal juſt reduced from the ore, which is the moſt unrefined and imperfect ſtate it can be in, is notwithſtanding real ſteel; whereas bar-iron, refined to the higheſt degree for common uſe, is in the greateſt degree remote from that character, and the more or leſs ſo, as it is more or leſs refined.

But notwithſtanding Mr. Reaumur and myſelf ſeem perfectly agreed, that iron-ore reduced to pig-metal is really ſteel, though in a ſtate of great imperfection; yet the queſtion with me is, how it comes to be ſteel?

Mr. Reaumur in his ninth Memoire, p. 239. in order to give us his idea of the matter, has recourſe to what he had advanced in his firſt; That iron in its mineral ſtate, is a compoſition made up of earthy particles, ferrugineous particles, and particles of ſulphur, and ſalt. He proceeds to inform his readers, that ſalt and ſulphur abound in all metallic mines; that the ſulphur muſt be ſublimed and carried off to a certain degree, before the metalline parts can be properly ſeparated. This, he ſays, is the caſe with iron ores, as well as with the ores of other metals; which would be liable to great waſte of metal, if the workmen were not to begin with roaſting the ore, or burning away by a gentle fire part of the ſulphur.

All this is readily granted: but ſtill, I apprehend, it does not quite come up to the point. There is no doubt that the ores of all metals in their mineral ſtate abound in ſulphur, and require this crude ſulphur to be carried off, in order to reduce their reſpective metals to ſuch a ſtate of purity and perfection, as will render them fit for their ſeveral uſes. With regard to different metals, different methods are purſued to obtain this effect. But copper and iron appear to be the two moſt difficult metals to be brought to this ſtate of neceſſary perfection. The former, frequently, requires ſeveral very elaborate operations for this purpoſe; though I have more than once reduced it by one very ſimple manner of treatment. After the firſt reduction in the common manner of working, it becomes what I think they call black copper; and then it paſſes through another operation to render it fine copper, fit for uſe. But my concern is not with copper, but with iron and ſteel.

Mr. Reaumur is of opinion, and endeavours to maintain it with great ingenuity, that as the iron-ore contains a very large proportion of ſulphureous and ſaline matter in its original compoſition, it is owing only to a partial removal of this original ſulphur, that, in the ſucceſſive ſtages through which it paſſes, it aſſumes different qualities, and becomes entitled to different characters.

Here I muſt beg leave to differ from him: For in my opinion, it is not altogether (if at all) owing to the remains of the original ſulphur exiſtent in the ore, that the metal aſſumes different qualities in the ſucceſſive operations through which it paſſes, but to the introduction of a more kind and genuine ſulphur, diſpenſed by the fuel made uſe of in the operation, (I mean if the fuel be charcoal) and forced among the metallic atoms by the energy of a violent fire; which either ferrits and drives out the original crude ſulphur, and immediately fills its place; or by its ſalutary virtue, ſo far corrects and alters its nature, as to render it innocent and inoffenſive. At the ſame time, I am perſuaded, the influence of the charcoal, in the firſt operation of running down the iron at the furnace, as truly converts the iron into ſteel, as the bar-iron is afterwards converted into ſteel, by being enveloped in a bed of materials proper for the purpoſe, and lodged together in a furnace as well adapted to carry on and finiſh the operation. This perhaps may be made more fully to appear, when we come to treat of that manner of converſion. But when the ore is firſt run down at the furnace, the operation being promoted there in ſo violent a manner, by the ſtrong blaſt of ſuch enormous large bellows, the ore and the fuel are ſoon torn to pieces with ſo much impetuoſity, as to be inſtantly reduced, both the one and the other, into a ſtate of fuſion; when, the former ever eager to imbibe any freſh ſulphur thrown in its way, and the latter as eager to perform its office, one would imagine the ferment and ſtruggle ſhould be ſufficiently powerful, for the vigorous efforts of the freſh ſulphur to diſlodge the former foul inhabitant, and take poſſeſſion of its place. And this poſſibly might be more effectually done, if there was no lime-ſtone made uſe of to promote the fuſion.

Taking this for a true ſtate of the matter, it muſt neceſſarily follow, that the ſteel, thus firſt produced, muſt be ſteel converted to a very high degree, and conſequently very hard and fragile; which is well known to be the caſe.

But in order to ſhew that in the whole of this I have given a juſt account, I muſt bring the matter to the teſt of experience.

In the numberleſs trials which I have made in miniature, of reducing iron-ore to pig-metal, I have generally been obliged, in order to preſerve my crucibles from being torn to pieces, by the voracious quality of the ſulphur exiſting more or leſs in all the varieties of mine-ſtone upon which I have made my experiments, to uſe a certain portion of charcoal duſt, the quantity of which I have been ever forced to vary according to the nature and quality of the ſulphur, which I met with in the different mine-ſtone. For this reaſon I have frequently been obliged to make the ſame experiment two or three, and ſometimes four times over; but have always found, the larger quantity of duſt I made uſe of, the reſult of the reduction was ever a more perfect metal, more particularly in two intereſting experiments.

The firſt was that of the American black ſand, an account of which was a few years ſince laid before the Royal Society, in a letter to my late worthy and ingenious friend Mr. John Ellicott, and by them ordered to be printed in the Tranſactions. The ſulphur with which this ſand was exceedingly fraught, was of ſo fixed, and at the ſame time of ſo corroſive a nature, that in one inſtance upon uſing a pretty powerful flux, the metal was all ſublimed and carried up the chimney. In another, where the flux was more moderate, the ſulphur carried off the metal another way, by gnawing the ſides of the crucible through. But being certain that the ſand contained a conſiderable quantity of metal, in order to prevent this laſt inconvenience, I charged the crucible with ſuch a quantity of the duſt, that it was difficult, even by the help of the flux, to collect the ſcattered atoms of the metal. However, by a little patience and perſeverance, I at laſt happily effected it, and was both pleaſed and ſurpriſed to find, out of about four ounces of the ore, two ounces of the metal, (though I apprehend not all collected) at the bottom of the crucible; but ſo changed, that inſtead of being fine pig-metal, which I expected, it turned out to be tolerably fine malleable ſteel.

But I ſhall not enlarge upon this, as I have reprinted that letter, and prefixed it to the preſent eſſay. Nor ſhall I here mention another experiment as much in point, as this juſt now recited; ſince I ſhall be under a neceſſity of relating it hereafter.

Now if there is reaſon to believe, that the firſt running down of the ore at the furnace, is really a converſion of the ferrugineous atoms into a very fragile immalleable ſteel, I think, it will naturally follow, that in carrying on the farther operations of refining the metal at the forge called the Finery, where the ſame ſort of fuel is continued, and the blaſt of a more gentle nature; it will follow, I ſay, that the change muſt by gentle degrees produce ſuch ſalutary effects, that at a certain period, could the blaſt be equally applied to every atom of the metal, as is the caſe in fuſion, the reſult would be, that the operator would be in poſſeſſion of a quantity of ſteel of a uniform texture, but greatly inferior to ſteel converted from good bar-iron; for obvious reaſons, which will come in courſe to be taken notice of by and by.

Mr. Reaumur in the above as well as in ſeveral others of his Memoirs, drops very inſtructive hints with regard to the ſteel made in Germany, as well as in ſome parts of France, from pig-metal not reduced to bar-iron, and then converted into ſteel; though at the ſame time he informs his readers, that a more perfect account of that matter is reſerved for another work.

It is to be noted, that this author, in his firſt Memoir, propoſes to his readers three methods of making ſteel; the two firſt are the methods uſed in Germany, and ſome parts of France, of making ſteel directly from pig-metal, without its being firſt reduced into bars; his third and laſt method, is that of converting bar-iron into ſteel: and a very odd medley of materials he recommends as proper to be made uſe of for that purpoſe; ſuch as ſoot, wood-aſhes, powdered charcoal, and ſea-ſalt; and theſe he directs to be mixed up in different proportions, according to the nature of the different ſorts of iron to be wrought upon. As he profeſſes to found his enquiries upon the practice of caſe-hardening, I am inclined to apprehend that ſome famous practitioner in that way furniſhed him with this notable recipe. The materials may, for any thing I know to the contrary, anſwer very well for the purpoſe of caſe-hardening; but I am not a little ſurpriſed, that ſo great a philoſopher, ſhould ſuffer himſelf to be ſo far impoſed on, as to recommend ſuch a jumble of materials for the purpoſe of converting bar-iron into ſteel: eſpecially as in the courſe of his experiments, he had met with ſeveral ſingle things that would ſo well anſwer the ſame purpoſe, and even mentions the very identical matter generally (if not always) made uſe of.

But to bring the matter, as to this point, to a final iſſue, I aſſure my readers, that powdered charcoal alone, or what they generally call charcoal duſt, is abundantly ſufficient to produce this valuable effect, of converting bar-iron into excellent ſteel. This, and this alone, is made uſe of at all the ſteel-furnaces, wherever I have been; and I am fully confident that, if this ſimple uſeful material were laid aſide for Mr. Reaumur's compoſition, it would be attended with very diſagreeable conſequences.

The furnaces made uſe of for this purpoſe are of different ſizes; ſome capable of converting only three or four ton-weight, while others are capacious enough to contain from ſeven to eight or ten tons. The outſides of theſe furnaces riſe up in the form of a cone, or ſugar-loaf, to the height of a very conſiderable number of feet. In the inſide, oppoſite to each other, are placed two very long cheſts, made either of ſtone, or of bricks capable of bearing the ſtrongeſt fire; which is placed between the two cheſts. The bars of iron, after the bottom is furniſhed with a neceſſary quantity of charcoal duſt, are laid in, *ſtratum ſuper ſtratum*, with intermediate beds of the charcoal duſt; to ſuch a height of the cheſts, as only to admit of a good bed at top; which is then all covered over, to prevent the admiſſion of the common air; which, could it procure an entrance, would greatly injure the operation. The iron being thus ſituated, the fire is lighted; which is ſome time before it can be raiſed to a ſufficient degree of heat, to produce any conſiderable effect. After which it is continued for ſo many days as the operator may judge proper; only now and then drawing out what they call a proof bar. This is done by openings, fit for the purpoſe, at the ends of the cheſt, which are eaſily and with expedition ſtopped up again, without occaſioning any injury to the contents left behind. When the operator apprehends the converſion is ſufficiently compleated, the fire is ſuffered to go out, and the furnace, with its contents, is left gradually to cool. This may take up ſeveral days: after which the the furnace is diſcharged, by taking out the bars of ſteel, and the remainder of the charcoal duſt.

The appearance of the bars, as well upon the ſurface, as in the interior parts, abundantly diſcovers the prodigious efficacy of this ſimple material, when urged by a ſtrong and ſufficiently continued fire. For the ſurface exhibits a great number of tumors, or bliſters, which give it the denomination of Bliſtered Steel.

Theſe excreſcences, which we call Bliſters, Mr. Reaumur terms Bulles, or Boüillons; and apprehends they may be owing to a ſort of boiling, or to a kind of motion ſimilar to that of boiling, made in the iron; but a little farther on he intimates, that theſe ſwellings may be owing to ſome parts of the iron not being ſo well cloſed, and rendered compact, while the bars were under the diſcipline of the hammer at the Chafery. But then it remains a query, Why that ſhould be the caſe? To which, I think, a very ready anſwer may be given; for as the foul ſulphur, with which the iron frequently abounds, is not ſufficiently purged out at the Finery, thoſe parts of the bars where it remains, are with difficulty brought to unite by welding, as it is called; and this remaining ſulphur being ſtrongly urged, in the converting furnace, by the ſtrong fire, joined with the agency of the more pure ſulphur exiſting in the materials made uſe of; muſt of courſe occaſion a very fierce conteſt, between the two combatants; the one endeavouring to expel the other; which being very difficult entirely to effect, it is natural to ſuppoſe the conſequence muſt be, that thoſe parts of the bars which had not been ſufficiently united, may ſuffer a partial ſeparation, ſo as to raiſe theſe bliſters. What tends to confirm this ſuppoſition, is, that when theſe bars of bliſtered ſteel happen to come under the hands of a judicious workman; before he attempts to bring them under the hammer, in order to work them down for any requiſite purpoſe; his firſt buſineſs is with a punch, to pierce theſe bliſters, (ſome of which I have ſeen near as big as a pigeon's egg) in order to give vent to the contained ſulphur, that the iron may be in a ſtate, to bear a welding heat, ſufficient to bring the parts into a proper union. I have frequently obſerved, that when theſe punctures have been made, the air and ſulphur have ſuddenly ruſhed out with a ſmart whizzing noiſe.—Thus much for the change brought about by the converſion upon the ſurface of the bars.

It now remains to make ſome few obſervations, upon the change wrought, by the operation, in the interior parts of the metal.

Mr. Reaumur is very particular, in his ſixth Memoir, upon this part of the ſubject; where he juſtly remarks, that, as the iron made uſe of, is more or leſs adapted by its ſtructure to be converted into ſteel, the ſteel in conſequence will exhibit very different phænomena. It ſhould be remembered, that Mr. Reaumur, and myſelf, are perfectly agreed, what ſort of iron is beſt adapted, for being converted into good ſteel; I mean, as to its texture and appearance; namely, that that ſort is the moſt proper, which is ſo far purged and refined, as to be formed into long fibres. During the time of the converſion, thoſe fibres are cut aſunder, and after the converſion ſhew themſelves in large ſhining maſſes; theſe large maſſes, one might expect, would, upon being reduced into ſmaller bars or rods, be again ſtretched out into ſmaller fibres. But the change made in the metal, rendering this impoſſible, it may be aſked, What is the circumſtance attending this change? A circumſtance, which, though very intereſting, appears to have eſcaped this curious and inquiſitive writer; but in the courſe of my buſineſs I have been led to obſerve it. It is this: that upon breaking a well hammered rod of this ſteel; inſtead of breaking off at right angles, with a faint rotten kind of noiſe in the rupture, (which is the caſe attending the common ſort of ſteel, converted from ordinary, or indifferent iron;) this breaks into a long flaky kind of grain, made up of a longiſh congeries of very fine atoms; and upon breaking occaſions a conſiderable concuſſion of the air; which the workmen term a breaking with a twang.

This ſort of ſteel is much better adapted to the purpoſe of making fine-edged inſtruments, as razors, lancets, &c. as I ſhall have occaſion more fully to ſhew, when I come to treat of ſteel more compleatly refined by fuſion, under the influence of a proper flux.

There are two other remarks of Mr. Reaumur, and others, viz. That the iron, in paſſing under this amazing change of being converted into ſteel, acquires a conſiderable augmentation both as to its volume and weight.

As to the former of theſe, viz. increaſe of volume, this ingenious writer obſerves, that what occaſions the bliſters, or Boüillons, as he terms them, upon the ſurface, muſt at the ſame time occaſion ſomewhat ſimilar through the whole body of the bar; that the effect is ſcarcely ſenſible, as to the breadth or thickneſs; but diſcovers itſelf as to the length; inſomuch that in a ſmall bar, only five inches long, it was increaſed one ligne and a half. As to the increaſe of weight, he makes the amount to be only $\frac{1}{256}$ part of the whole; in which calculus I apprehend he muſt be miſtaken; as a gentleman in the north, maſter of a large ſteel-work, has aſſured me, that he eſtimated his increaſe to be one hundred weight upon ſeven tons.

I have before allowed an aſſertion of Mr. Reaumur's to be juſt, viz. That pig-metal is really ſteel, but of a very harſh fragile nature; though I differed from him as to the real cauſe of its meriting that character, viz. Whether it is owing to the raw ſulphur originally inherent in the ore, or to the ſulphur of the charcoal made uſe of in its reduction in the furnace. In order to put it beyond doubt, that the latter is the caſe, I procured ſome of the moſt imperfect ſort of Ruſſia bar-iron, termed Brinſky-Iron. This ſort of iron is well known by judicious workmen, to be a compound, of pretty nearly equal parts, of raw pig-metal, and iron tolerably well purged; or, according to Reaumur's opinion, half iron, and half ſteel: I melted a ſufficient quantity of this metal, and run it into an ingot; the conſequence was, I had a ſmall bar of low converted ſteel, but of an uniform texture; which by proper management however, I had wrought into a dozen of tolerably ſerviceable table-knives. A ſtubborn proof this, that running down in the ſmelting-furnace really converts it into ſteel.

I ſhould now have proceeded to perform the promiſe I made at my firſt ſetting out, namely, to ſhew the manner and utility of reducing bar-ſteel into a more compact and uniform texture by fuſion; were it not for the propriety of firſt taking notice of an extraordinary pamphlet which hath lately appeared, written by a famous artiſt in manufacturing ſteel at the weſt end of the town; and followed by an advertiſement as extraordinary,

dinary, in the Daily Advertiſer, of Wedneſday, Jan. 8, 1772. The title of the pamphlet is, “ An Eſſay on the Myſtery “ of Tempering Steel; wherein the Ef- “ fects of that Operation are fully conſi- “ dered. Said to be extracted from the “ Works of the celebrated Monſ. Reau- “ mur.” The advertiſement intimates a diſcovery being made by this ingenious artiſt, “ of a method of tempering thoſe “ fine-pointed inſtruments called lancets, “ in ſuch a manner as to obviate a gene- “ ral complaint of the liableneſs of ſuch “ exquiſitely fine points breaking in the “ operation.”

If ſuch a complaint has been general, to be ſure, finding out a method to prevent it, muſt be looked upon as a very uſeful diſcovery. But I never happened to be informed before, that it had been general: indeed my own experience muſt bear witneſs to the contrary; for though I have had great numbers of thoſe curious inſtruments made from my own ſteel, I never heard of one inſtance to favour ſuch an opinion. Indeed happening lately to be in company with a gentleman not long ſince arrived from New-York, he informed me, that in being blooded he had once met with ſuch an accident, the point of the inſtrument having broken in the vein, and been diſcharged by the violent impetus of the blood, iſſuing from the orifice. But being, upon enquiry, informed that the lancet was made by a New-York cutler, my ſurprize was a little abated.

Upon a careful peruſal of the eſſay above mentioned, and looking over the particular Memoir of Mr. Reaumur, from whence the pamphlet appears to be extracted; which before I happened to have taken little notice of; I was concerned to find, what great pains had been taken to make that a myſtery, which had before always appeared extreamly obvious, as a matter of fact, and eaſily to be accounted for. Mr. Reaumur, I own, has not been ſufficiently clear and explicit; having blended together two or three ideas under the ſame term, viz. that of tempering of ſteel. His tranſlator, as he was writing for the information of Engliſh artificers, ſhould have been careful to have delivered his ſentiments in ſuch terms, as were familiar to them, and as they could not eaſily have miſtaken. But this, even at his firſt ſetting out, he appears to have neglected. He begins thus: "The diſtinguiſhing character of ſteel, conſidered with reſpect "to the offices it ſerves, conſiſts in the "peculiar property it has of becoming "hard by means of tempering."

Here the writer confounds two very expreſſive words appropriated to very different notions; for judicious workmen generally make uſe of three capital terms, in order to preſerve a proper diſtinction of ideas, viz. Hardening, Tempering, and Nealing. The firſt idea is defined above, under the term, Tempering; but improperly, for Tempering means reducing the ſteel into a ſtate between hard and ſoft; which admits of different degrees, according to the different purpoſes for which the ſteel is required to be uſed; and is brought about by adminiſtering a different degree of heat. The laſt, Nealing, is produced by heating the ſteel red-hot, and leaving it, in that ſtate, gradually to cool of itſelf; which reduces, and brings it back, to the condition it was in before it was hardened. By cloſely adhering to theſe terms, we ſhall be able, under every ſtage, to keep our ideas perfect and diſtinct, without running into errors, either on the one hand or the other; or being loſt in myſtery.

The author of the eſſay, in page 4, diſcovers, as he himſelf informs us, the whole of the myſtery in the matter of hardening; which, ſays he, "may be "explained in very few words: A piece "of ſteel is heated, and becoming red, "is ſuddenly plunged in cold water. "This little proceſs," he adds, "is ſuf-

" ficient to have given a conſiderable de-" gree of hardneſs to the piece of ſteel, " which had it been left to remain and " cool among the coals from which it re-" ceived the heat, would have been ſtill " in a ſoft ſtate:" that is, it would have been nealed. As for the myſtery, it conſiſts, I ſuppoſe, in what he next remarks; namely, that though the ſteel, when, without being plunged in the water, it is left to cool among the coals, continues in a ſoft ſtate; yet were we " not pre-adviſed " of the fact, but left to gueſs by which " circumſtance the ſteel had acquired the " greater hardneſs, we ſhould be tempted " to pronounce in favour of the latter." On the contrary, the probability appears to me to be in favour of the former. And when I come to aſſign the cauſe of ſteel becoming hard by being quenched in water, I believe it will not appear ſo myſterious as he imagines. In the mean time, I ſhall conſider the cauſe which he aſſigns.

By the “ heat,” ſays he, “ we give “ the ſteel in tempering” (hardening, he ſhould have ſaid) “ we introduce into it “ innumerable particles of fire, which ſe- “ parate and detach its parts, and, in “ a word, enlarge its dimenſions. In “ this ſtate,” he obſerves, “ it is pre- “ cipitated into cold water: the water “ very ſoon puts a ſtop to the action “ of the fiery matter, which had pene- “ trated the ſteel; hence a part of the “ augmentation which the fire had oc- “ caſioned, becomes fixed in the maſs.” That this is not really a true ſtate of the caſe, is extremely evident, from the hiſſing, and even the boiling of the water, if the body immerſed in it be of any conſiderable ſize; which ebullition continues for ſome time, but gradually declines, till at laſt it entirely ceaſes; yet not till every atom of the fire is expelled, and forced off through the water. I am very ready to allow that the ſteel has an increaſe as to its bulk, during this operation: but this is owing to quite another cauſe than that here aſſigned; which will appear, and even be allowed by Mr. Reaumer himſelf, farther on.

He comes in p. 7. to prove, the increaſed dimenſion of ſteel by hardening, from the examination "of a piece of "ſteel that is broken after having been "hardened, which," he ſays, "will be

“ found to be more porous than before:” and in order to this, he adds a note, extremely curious; namely, “ This aſſer-“ tion muſt at firſt ſight appear extraor-“ dinary,” (indeed it muſt!) “ as thoſe “ who have hardened ſteel, never think “ that operation well performed, unleſs “ the grain of the ſteel appears to be “ much cloſer than that in a ſoft ſtate.” It is true, every judicious workman is really of that opinion; and cannot well entertain a different one, whilſt he hath eyes for obſervation.

But what is the criterion by which this extraordinary poſition is proved? Why, by a criterion, in my opinion, as extraordinary as the poſition. For he proceeds thus, imagining no doubt that he is ſtill following Mr. Reaumur: “ The

" body of ſteel hardened," ſays he, " ſeems " of a texture much leſs porous than that " which is not hardened; its grain is, " nevertheleſs, conſiderably larger, and " its appearance of cloſeneſs is owing " entirely to the fluid matter which the " heat had diſperſed among its particles, " and which became fixed in the inter- " ſtices of the grain, by the ſudden man- " ner of cooling it." And if any reader ſtill remain doubtful, he is brought to an experiment, which is to cure him of his infidelity. " For he need only quench " a piece of ſteel that is heated much " more than common, and he will find " the grain extremely coarſe; for the vio- " lence of the heat having driven the " fluid matter from among its interſtices, " the ſize thereof will then be very diſ- " cernable."

This experiment among workmen is termed burning of the ſteel, or rendering it unfit for uſe, without ſome farther operation to reſtore it to its priſtine texture. Many an inſtrument have I known thrown away as entirely ſpoiled, when this experiment has happened by accident or careleſsneſs.

However, as an experiment, it by no means proves what it is brought for: all it proves is, that if a body of ſteel be conſiderably heated above what is abſolutely neceſſary to give it a proper hardening; the grains, atoms, or fibres, call them by what name you pleaſe, are ſo far ſeparated, by the exceſs of heat, that plunging them in that ſtate in cold water, will not afford a ſufficient degree of counteraction, to bring them together again, into ſo compact a form as to entitle them to the denomination of fine-grained hardened ſteel. And I think it extreamly odd, that diſtorting a body of ſteel by giving it an over-heat, and thereby a coarſe unconnected appearance, when hardened, ſhould be brought as a proof, that ſteel, hardened in the proper method, occupies more ſpace than before; when reaſons much more concluſive, and more philoſophical, may be brought to prove the ſame fact.

In excuſe for ſpending ſo much time in conſidering this note, and this part of the eſſay, I beg leave to make this uſeful remark: It has been time out of mind a prevailing opinion, even amongſt the moſt experienced workmen, that when any inſtrument, a graver for inſtance, is thus over-heated, it is entirely ſpoiled; and accordingly, as before obſerved, it is frequently thrown aſide as uſeleſs; and if the ſize of the inſtrument is ſufficient to admit it, it is cuſtomary to carry it to the forge, give it a freſh hammering, to cloſe the pores; and then to harden it again, at a more moderate heat. But I can aſſure my reader, that there is no occaſion either to throw away, or to re-hammer, an inſtrument thus apparently injured; for if in this ſtate it be again committed to the fire, a proper heat for hardening adminiſtered, and it be then plunged into the water afreſh, the atoms will be brought together again in the moſt cordial manner, and form as fine a texture, as can be deſired; and yet the ſteel will occupy more ſpace than it did before it was hardened.

The truth of the caſe is, that if the heat be too low, the ſteel will not receive a ſufficient degree of hardening by being plunged in the water; if it be overheated, the parts will be removed to ſuch a diſtance from each other, as to render the check it receives from the plunging in the water, inſufficient to reſtore them to a proper contact, ſo as to render it fit to form a fine inſtrument: but neither in the one caſe, nor the other, is the ſteel itſelf really injured, the conſtituent parts remaining ſtill the ſame; ſo that if it be heated a third time to a proper degree only, and then replunged, it will receive as fine a grain, as if the firſt heat had been adminiſtered with all imaginable accuracy. This ſecret I was acquainted with, many years before I knew any thing of Mr. Reaumur: I have yet, ſo far as I can recollect, communicated it only to two ingenious mechanics; but having the preſent opportunity, I thought it my duty to make it public.

I hope to be excuſed if I paſs lightly over the remaining pages of this performance, and leave my readers to an attentive peruſal of the eſſay itſelf, if they think it requiſite.

In page the 9th is only a trial made by Perault, retried by the eſſayiſt, to prove the enlargement of ſteel by tempering, as he calls it; a thing, which I apprehend, nobody denies.

The next page contains a calculation made by Mr. Reaumur, of the amount of that augmentation which ſteel acquires by hardening; namely, a 48th part at leaſt of its original ſize; which I preſume likewiſe, no one will controvert. The writer's inference, I own, I do not underſtand, viz. "That what he has before "offered ſufficiently proves, that tem-"pered ſteel is of a texture more rare "than when it is ſoft." If by more rare, he deſires to be underſtood, that the parts of the former are more ſeparated and detached the one from the other, than they are in the latter, I can by no means agree with him: and if the former experiment of over-heating be again brought as a proof; I cannot help ſaying, that it is really nothing more than a mere *deceptio viſus.*

After a number of experiments made to prove, that the increaſe of the hardneſs of ſteel, notwithſtanding the augmentation of its ſize, is not owing, either to the fire or water adding any matter to the ſteel, which occaſions the increaſe, and unites its parts better together; or to its driving any matter out of it, which prevented the parts from being ſo well united as they might otherwiſe be; all which experiments appear to me very inſignificant, notwithſtanding Mr. Reaumur himſelf ſeems to have given them a ſanction: after all this, I ſay, at the bottom of the 22d page, the writer brings the affair to a criſis.

He there obſerves what, he ſays, "is "well known, that the principal diffe-"rence between iron and ſteel is, that "the latter is very conſiderably more "impregnated with particles of ſulphur "and ſalt, than we find the other to be." Here then we are to find the preciſe difference between the two; and at the ſame time, the reaſon why, and the manner how, ſteel acquires the property of becoming hard. But how comes it to paſs that we could not at once have been brought to this criterion, without being led through ſo many obſcure and intricate windings? Would the myſtery, as he affects to call it, have appeared leſs ſurpriſing?

We are next reminded, that both iron and ſteel have an aptneſs to imbibe ſulphur, as they acquire heat: This is extremely well known, and is true not only with reſpect to iron and ſteel, but likewiſe every other metal; or copper would never be converted into braſs by being in a proper manner mixed with a due proportion of *lapis caliminaris*; nor would it become what is called bell-metal by a proper mixture with tin. But to return, the premiſſes being admitted, what is the conſequence? To determine this, we are to call in, it ſeems, the aid of the microſcope, in order to diſcover the component parts of ſteel; a ſingle grain of which, we are told, is made up of a collection of other grains, which we are to term molecules; and theſe appear likewiſe to be made up of other parts, which we are allowed to ſuppoſe are of an elementary nature. And he tells us, p. 24, that "to carry our ideas ſo far as the "ſubject would permit, we might ſtretch "this diviſion much further; but it may "be ſufficient for the preſent purpoſe to "let it reſt here." And then he informs us, what uſe he makes of this minute diviſion, in order to explain more fully, " how " ſteel becomes hard, when it is cooled " ſuddenly, and why it is ſoft, when it is " ſuffered to cool by ſlow degrees." His words are theſe, which I quote at length, that I may not, by abridging, miſrepreſent them: " We have, ſays he, under " our conſideration a ſingle grain of ſteel, " the molecules it is compoſed of, and " the elementary parts of thoſe molecules. " Now, in expoſing to the fire the piece " of ſteel which contains the grain (at " preſent the object of our conſideration) " we ſhall find, that the ſulphur and ſalts, " contained in the molecules, will be " ſooner melted than the molecules them" ſelves, and are by the heat driven from " among the parts in which they were " contained, and take place amidſt its

“ vacancies. Thus the matter, which the
“ heat had expelled from within the mo-
“ lecules, now fills up the ſpaces between
“ them: it is pretty evident then, that,
“ when a grain of ſteel has received a
“ certain degree of heat, the vacant ſpaces
“ of the molecules, of which it is com-
“ poſed, are in part filled up by a ſul-
“ phurous matter, which they did not
“ before contain, and which had been
“ extracted from the molecules them-
“ ſelves. In this ſtate the piece of ſteel,
“ which contained the forementioned
“ grain, is ſuddenly plunged in water,
“ and, in that inſtant, we fix at once the
“ ſulphur and ſalts, which were in a ſtate
“ of fuſion, and thereby depriving them
“ of their fluidity, they are no longer in
“ a condition to re-enter the cells, from
“ which they were driven. And thus,”

ſays he, "the ſmall interſpaces of the "molecules are better filled up, and by "a matter which we may ſuppoſe almoſt "as hard as we pleaſe." But is it not obvious to remark, that if the interſpaces between the ſeveral molecules are thus filled up by a ſulphurous and ſaline matter, which is derived from within the molecules themſelves, and which muſt be ſuppoſed to be, as it were, the cement, which unites the elementary parts of the molecules together; then in proportion as the exterior parts of the ſeveral molecules are more united to one another, and become harder, the interior elementary parts, being deprived of this cement, will become leſs united with each other, and be more eaſily ſeparated. All the effect, therefore, which I can perceive likely to follow from a different poſition of the ſulphurous and ſaline matter, which is ſuppoſed to be contained in the ſeveral molecules, will be the diſſolution of the old molecules into their elementary parts, and the forming new molecules of other elementary parts, united by that ſulpurous and ſaline matter, or cement, which, before it was driven out of its place by the heat, united the parts of the former molecules. In ſhort it appears to me impoſſible, to account for the hardening of ſteel, merely by the removal of the ſulphurous and ſaline matter from one part of it to another; ſince what is thus gained in one part, will be loſt in the other. I apprehend, therefore, we ſhall be obliged to have recourſe to the introduction of a certain quantity of ſulphurous and ſaline matter, in order to account for this phænomenon: of which I ſhall treat more largely hereafter.

The writer, p. 29, not very happily indeed, falls upon caſe-hardening, as the fitteſt ſubject to exemplify his ideas; adding a remarkable note, to explain what is meant by caſe-hardening. " This operation," ſays he, note, p. 30, " is termed " by thoſe who perform it, caſe-hardening, " and with much reaſon, as the thing, " which is to be hardened, is heated in a " caſe, uſually made of iron." This happens to be *un faux pas*; and I hope the gentleman will not take it amiſs, if I endeavour to ſet him right. The term then is made uſe of to intimate, that by the operation (which he explains tolerably well) it is intended to cover the ſubject, whether it be a bar, or any particular inſtrument made of iron, with a coat, or caſe of ſteel, thicker, or thinner, at the pleaſure of the operator, as the operation is continued a longer, or a ſhorter time: and he might as well, for the reaſon he gives, have called converting bar-iron into ſteel, caſe-hardening; ſince the bars to be converted, are likewiſe confined in a caſe.

A little farther on (p. 32.) he remarks, that " This method" (namely the operation of caſe-hardening) " uſed even with " iron, occaſions a hardneſs about its ſur- " face, approaching to that of ſteel," and as he expreſſes himſelf afterwards, " nearly equal to that of ſteel." I muſt beg leave to inform him, that the hardneſs thus occaſioned, is quite equal to that of ſteel; and that the ſurface of the metal is really ſteel.

In a note in the preceding page he ſaith, " This proceſs, however, is com-

“ monly uſed with iron only, ſteel being
“ capable of receiving a ſufficient degree
“ of hardneſs by the methods ordinarily
“ practiſed for that purpoſe.” A very odd remark this! What, are there two different methods made uſe of, the one to harden an entire piece of ſteel; and another to harden a piece of iron caſed over with ſteel? For my own part, I never heard of more than one.

“ This proceſs, however,” he tells us, “ is commonly uſed with iron only.” But it has been uſed with ſteel alſo; and poſſibly I was myſelf the firſt that ſo uſed it. And I would here inform the eſſayiſt, that as a caſe of ſteel, may be formed upon a rod of iron; ſo a caſe of harder ſteel may be ſuperinduced upon a rod of already converted ſteel.

When making artificial magnets firſt became pretty much in practice, I among others was willing to have a ſtroke at it, and being as willing to excel, I uſed the method of caſe-hardening the bars; by which I found my bars really did excel. And the late Mr. Canton, who was ſo lucky as to hit upon a more expeditious method of communicating the magnetic virtue than had before been practiſed, was aſſiſted by me in procuring the beſt bars for his purpoſe. I gave leave to a very ingenious and worthy man, then in my ſervice, (without receiving any benefit myſelf) to employ his leiſure hours in forming and hardening bars for Mr. Canton, according to a method which I had directed, and with the nature of which I made that gentleman acquainted, upon his applying to me, previous to the publication of his account of the manner of making artificial magnets, in the Philoſophical Tranſactions. To this method of hardening the bars, the knowledge of which he received from me, Mr. Canton refers, when he *coolly* ſays, (I wiſh he had expreſſed himſelf in more friendly terms,) the ſmith that I *employed*, hardened them ſo and ſo.

If the eſſayiſt had followed his author into the next Memoir, he might have met with obſervations worthy his notice; and perhaps the more ſo, as ſome of them are flat contradictions to thoſe hints, from which he had drawn ſome eſſential concluſions. Particularly, whereas, p. 40, he ſpeaks of "ſteel becoming harder by "being tempered hotter," Mr. Reaumur obſerves, p. 344, that it has been an obſervation of ſome curious workmen, that when ſteel had been tempered, or hardened, at too hot a colour, it would not render it more, but rather leſs hard, than when heated to a more moderate degree; and I apprehend this to be the ſettled opinion of every one, who merits the name of a good workman.

I hope I ſhall be excuſed having led my readers ſo far out of the way in queſt of an imaginary myſtery in tempering of ſteel: I ſhall now lay before them briefly the true ſtate of the caſe; that if there be any myſtery, we may be able to give it a proper inveſtigation.

I allow with Mr. Reaumur, that when the iron-ſtone is firſt taken from the earth, it brings along with it a very foul ſulphur; to remove which in ſome meaſure, before it enters the ſmelting-furnace, they are obliged to give it a ſtrong torrefaction; in which operation not only a great part of the ſulphur is expelled, but the body of the mineral, (as before obſerved) is ſo far opened, and the ſtubborn texture of the compound, occaſioned by the crude ſulphur which ſtrongly binds it together, is ſo far relaxed, that when it comes into the furnace, the remainder of its natural vitious ſulphur ſuffers an eaſy expulſion, by the large quantity of charcoal made uſe of in the operation, aſſiſted by the ſtrong blaſt of the bellows; while at the ſame time the more kindly ſulphur of the charcoal ſupplies its place, and converts the metal into a very hard frangible ſteel.

In the ſucceeding operation at the forge called the Finery, if any of its original ſulphur yet remain, of which I apprehend there can be only a very inconſiderable quantity, that likewiſe ſuffers a very eaſy ſublimation; but it happens at the ſame time, that not only the remainder of this crude ſulphur, if there be any, but likewiſe a very conſiderable quantity of the more ſalutary ſalt and ſulphur, which it had recently imbibed from the charcoal, unavoidably makes its eſcape, along with it; becauſe, (notwithſtanding the fuel ſtill made uſe of be charcoal,) the operation is performed in a more gentle manner, and upon an open hearth.

In the courſe of this operation, the metal through every ſtage for a conſiderable time ſtill retains the character of ſteel; but of a higher or lower degree, in proportion to the quantity of ſulphur, that has in this manner been carried off, till at laſt it becomes malleable ſteel: all this Mr. Reaumur allows to be true; and he farther obſerves, that could the metal be in every part uniformly and equally applied to the blaſt, it might at a certain period be deemed tolerably good ſteel. But as this equality is in this manner very difficult, if ever, to be obtained, it has been judged neceſſary to carry on this operation ſtill farther, to reduce the metal to fine malleable bar-iron, in order to its being in a proper condition, to be reconverted into bar-ſteel.

But how is this recovery, or reconverſion, to be obtained? Why ſtill by the medium and miniſtry of charcoal: and the manner in which this is effected, is ſhewn in a former part of this eſſay, p. 109, &c. where the nature of the furnace, in which the operation is performed, and the proceſs itſelf, are fully deſcribed, as well as the changes, both external and internal, which are made upon the metal.

And now as to the myſtery of tempering, from every obſervation that can be made between the different natures of iron and ſteel, I am obliged to ſubſcribe to what Mr. Reaumur himſelf has advanced upon the ſubject, viz. That one differs from the other merely in being impregnated with a larger proportion of ſalt and ſulphur. At his firſt ſetting out, in his treatiſe upon the art of converting bar, or forged iron into ſteel, he affirms, as I have frequently obſerved, that the mines of iron are compoſed of ferrugineous parts, of earthy parts, and of parts of ſulphur and ſalt. It has been already ſhewn, that a method hath been diſcovered to ſeparate the metallic parts, or ferrugineous atoms, from the heterogeneous matters with which they are mixed, and to reunite, as Mr. Reaumur obſerves, the parts diſperſed into a proper maſs, to render the metal fit for its ſeveral different uſes. He proceeds to obſerve, that fuſion is the firſt mean employed for this purpoſe.

Now if from what has been offered, we may venture to take it for granted, that at the firſt running down of the mine, by means of the large quantity of charcoal made uſe of in the operation, the iron is converted into a very hard immalleable, and to the higheſt degree frangible, ſteel:

which Mr. Reaumur himſelf allows to be the caſe, and even makes it the firſt term of his gradation, when enumerating the various degrees of ſteel in reſpect to its different hardneſs: and if this difference, as he allows, be owing to one degree being impregnated with a larger proportion of ſalt and ſulphur than the other; from its ſtate of pure malleable iron, with only ſo much ſulphur left as is ſufficient to form a vinculum, to hold the parts of the metal in a proper degree of coheſion, to the higheſt term in his gradation: I ſay, allowing this to be true, I cannot help being of opinion, that every degree of the converſion of bar-iron into ſteel, from the loweſt term to the higheſt, carries on in the metal a ſucceſſive approximation to the nature of a ſemi-vitreous body. Pig-metal, as it is called, is almoſt as hard, and nearly as fuſible as glaſs. Bar-ſteel highly converted, and formed into a proper inſtrument, will cut glaſs almoſt as readily as a diamond. A bar of high converted ſteel being broken, and compared with a bar of pure iron, the former exhibits a very different appearance from the latter; the one is eaſily brought into fuſion in the crucible; the other, with the greateſt difficulty; and ſcarcely at all, if very pure. I have myſelf frequently made the attempt; but without ſucceſs; as I have ſcarce ever been able to reduce it to a ſtate of fluidity; inſtead of which, I have been able only to tear the metal to pieces, by ſuch a diſunion of its parts, that when I have attempted to run it into an ingot, it has run out of the crucible moſtly in a fine ruddy powder, with here and there a few ſmall maſſes clinging together very looſly. And to compleat the whole, when it comes to be hardened, by being properly heated, and ſuddenly quenched in cold water, it becomes almoſt as brittle as glaſs itſelf; and by a proper admixture of other matters, aſſiſted by a ſlight previous calcination, it may with no great difficulty be run into glaſs. And here, if any where, I am fully of opinion we muſt look for the great myſtery (not of tempering, but) of hardning ſteel: which may be briefly explained in the following manner:

Steel is iron converted, as it is properly termed, by the introduction of a certain quantity of ſalt and ſulphur; which, being not only mixed and ſettled between the interſtices, but entering into, and even making a part of the minuteſt atoms of the metal; and being, at the ſame time, by a very moderate degree of heat eaſily brought into a ſtate of fuſion, ſwells and expands every atom of the metal; and the metal in this ſtate being ſuddenly immerged in cold water, every atom, with its contents, is as ſuddenly arreſted in this expanded ſituation, which, at the ſame time that it increaſes and fixes its bulk, renders it nearly as brittle as glaſs.

Having thus far (I hope ſatisfactorily) cleared the way, with regard to the myſtery, as it has been fondly termed, of hardening ſteel; I ſhall now ſubjoin a few conſiderations, with regard to the myſtery, if it muſt be ſo called, of tempering ſteel.

This among our Engliſh workmen is termed, and I apprehend not improperly, reducing or letting down, that is lowering, the nature of the ſteel, by adminiſtering ſuch different degrees of heat, as may be proper to qualify the inſtrument to which it is applied, for its intended uſe.

Thus, for inſtance, with regard to razors, lancets, and ſuch curious inſtruments, the cuſtom is, either by an open fire, or by a red-hot bar of iron, to apply the inſtrument to the heat, and hold it there till the proper colour, by which to judge of the heat given to the inſtrument, appears upon the ſurface, as perhaps a high gold colour; and then the inſtrument is again immerſed in the water, as ſuddenly as when it was at firſt hardened; this is done in order again to arreſt the phlogiſton in its then ſtate; as otherwiſe, the heat, remaining in the metal, would go on ſtill farther to reduce the temper of the inſtrument, till it would be diſqualified for the purpoſe intended. If the inſtrument be of a groſſer kind, the colour muſt be ſtill farther advanced, and the temper ſtill farther reduced, till the inſtrument be judged fit to endure the more arduous work it is intended to perform.

But as no ſubject, that I know of, requires a more accurate adjuſtment and preciſion in this particular, than the ſprings of watches; I ſhall ſingle out that, as one of the moſt intereſting, to give it a very cloſe and impartial conſideration.

The main ſpring of a watch is known to be a very thin plate of ſteel; in this form it is brought into a double ring, bound round with wire, and committed to a proper furnace, in order with great caution to give it a ſuitable degree of heat; when that is ſufficiently acquired, it is taken out, and dropped into a kettle of oil, or warm mutton ſuet; for water would prove too chilling; from hence being taken out, it has acquired a degree of hardneſs, very little inferior to that of glaſs. After this it is reduced, or let down, if the ſteel be of a proper kind for the purpoſe, to a fine violet, or blew colour; in which ſtate it becomes exceſſively elaſtic, inſomuch that to make it to ſtand bent, it muſt be brought into a very acute curve; nor can it eaſily be brought by twiſting horizontally to alter its form, without endangering its being rent to pieces. But as after this, it requires to be planiſhed by the hammer, filed and poliſhed, it again becomes deprived of its elaſticity to a very conſiderable degree; but when it has received its laſt degree of fine poliſh, it is again blued upon a braſs plate over a lamp, by which means its former elaſticity is perfectly reſtored.

What is here extremely remarkable is, that had the ſpring been wound up, and placed in its proper incloſure in the watch, without being again blued; it is much to be queſtioned, whether the watch would ever have meaſured time with any tolerable degree of exactneſs; yea, ſo abſolutely neceſſary is the retention of this blue ſkin, if I may ſo call it, upon the ſurface of the ſpring, that if ever, by the inequality of the ſpring, a more than ordinary

dinary friction ſhould happen among the turns, ſo as to rub off the ſkin in any conſiderable number of places, which I have frequently known to be the caſe; the elaſticity will become ſo vehemently diſturbed, that the watch will never afford any proper degree of ſatisfaction to the wearer, till the old ſpring be removed, and a new one placed in its room. The ingenious Dr. Hook, in his Micographia, taking particular notice of this fine blue covering, gives it, very pertinently, as his opinion, that it is owing to the fine ſulphur, preſſed through the pores by heat, and fixed by the ambient air in a fine vitrified lamina upon the ſurface.

It would, I think, be a very curious, as well as intereſting, query: What is the phyſical reaſon of the variation of elaſticity under this variation of circumſtances? the elaſticity of the ſpring, upon the firſt tempering of it, being prodigious; and in the operation between the tempering, and its acquiring the laſt fine blue covering, being very conſiderably diminiſhed; and, by introducing the laſt covering, being again amply reſtored and fixed.

Having now ſaid every thing, which I apprehend neceſſary, relative to the nature of iron; tracing it from its being firſt taken from the mine, through every ſtage, till it is reduced to good bar-iron; and having likewiſe diſcovered what change is wrought upon it, in its converſion into that ſtate, in which it aſſumes the denomination and character of ſteel;

and marked out the difference between the one and the other; I now proceed to conſider, as I propoſed, both hiſtorically and phyſically, the lately diſcovered method, of re-melting bar-ſteel in a crucible, and running it into a ſolid ingot; by which means, when the metal is properly choſen, and the operation duly performed, we are furniſhed with a more excellent kind of ſteel, for ſome important purpoſes, than was probably before known in England.

It is not a great many years ſince this diſcovery was firſt made, by a gentleman (as I have been informed) reſiding in the Temple, an acquaintance of the late Lord Macclesfield; whoſe name I could never learn; nor could I ever gain the leaſt information of the means, by which he became poſſeſſed of ſo valuable a ſecret. He however judged it proper not to let it remain a ſecret any longer, than till an opportunity offered of diſcovering it to ſome perſon, who, he imagined, might be able to render it of ſervice, in ſome ſhape or other, to himſelf, as well as to the world. While the gentleman was thus inclined, there fell in his way, one who had been employed in flatting of gold and ſilver wire for the uſe of the lace-men; whom he judged to be a proper perſon for his purpoſe.

Prior to this period, the workmen at a very conſiderable expence, and no ſmall riſk, had been obliged to ſmuggle thoſe implements, or ſteel-rollers, by which the wire is flatted, from Lyons in France; whereas our melted ſteel is qualified to form them in a much better manner; but ſtill liable to this conſiderable objection; that as the ſteel is obliged firſt to be forged into plates, there is an abſolute neceſſity of their being welded in one particular part, which not only injures the ſteel, when they are bent round the iron roller, but leaves a diſagreeable roughneſs upon that part, where the welding-heat muſt of neceſſity be adminiſtered.

To obviate this difficulty, the ingenious Dr. Lewes has propoſed a more advantageous method of forming theſe rollers, viz. By having them caſt in one entire piece, with a proper opening in the centre, by which means they may very commodiouſly bear the diſcipline of the hammer, in order to cloſe the pores, that they may become fit for being hardened.

Happening lately to fall in the way of my ingenious friend Mr. John Bird; who diſcovered ſome concern with regard to the difficulty of procuring a couple of ſteel collars, to turn upon the cylinders at the centers of the large quadrants, which he was than making for the new Obſervatory at Oxford, without being ſubject to wear by the friction; this hint of Dr. Lewes's ſo forcibly ſtruck me, that I immediately propoſed their being formed in this manner; the attempt ſucceeded ſo well, that I became thoroughly ſatisfied, Dr. Lewes's ſcheme might eaſily have been put in practice, could one difficulty have been ſufficiently guarded againſt, namely, the liableneſs there might be in the rollers to caſt, or fly in the hardening. This difficulty has ſince been overcome by Mr. Bird, the ingenious artiſt juſt mentioned. To return:

The perſon, to whom the ſecret abovementioned had been communicated, could not reſt ſatisfied with a vigorous purſuit of his own buſineſs, the flatting of gold and ſilver wire, which might in all likelihood have proved very advantageous to him; but muſt needs, though no competent judge of the nature either of iron or ſteel, commence a maker of razors; imagining, I preſume, that nothing more was neceſſary to form a good inſtrument of that kind, than its making a fine brilliant appearance. For this purpoſe, having melted a conſiderable quantity of ſteel, without being a judge whether or not, it was fit for the purpoſe; he applied to a very honeſt worthy man, one Mr. Humphrys, a cutler near Covent Garden, who for a ſuitable premium became his operator.

The razors, made from this ſort of ſteel, wearing, whatever was their intrinſic merit, a much finer face than common, procured him a pretty large number of cuſtomers at the weſt end of the town, where he became a conſiderable hawker. This circumſtance ſoon alarmed the cutlers in that part. Upon which old Mr. Savigny, a very ingenious artiſt in his way, the above named Mr. Humphrys, with ſeveral others whoſe names it would be needleſs to mention, applied to me; and at their entreaty, having built a furnace, I went to work; and though at firſt ſetting out I met with no ſmall difficulties, it was not long, before I was enabled to ſurmount them, and to furniſh my ſolicitous cuſtomers with a commodity, vaſtly ſuperior to what the perſon, who had riſen up in their way, had ſufficient ſkill to procure for his own uſe.

I and my new cuſtomers went on cordially for ſome time, as I had undertaken to ſerve them at a very moderate price. But it ſo happened, that our razor-maker, finding the large expectations he had formed in this new employment, broken in upon by my cuſtomers, who were qualified vaſtly to outdo him; ſoon fell upon a new ſcheme, by which he apprehended, he might both injure his competitor, and procure himſelf great advantages. For this purpoſe, he went into the North, to diſpoſe of his ſecret to the beſt advantage; firſt to Birmingham; and not finding ſufficient encouragement there, to Sheffield; and, as I have been informed, offered his ſecret to ſeveral conſiderable manufacturers, at ſo extravagant a price, that few or none cared to be purchaſers. However, he met at laſt with ſome keen friends, who wormed the ſecret out of him, ſupplied him with a little money, and ſent him back to town; and they, being better ſkilled in the nature of ſteel than he was, ſoon outdid their maſter. My new cuſtomers, now finding they could purchaſe melted ſteel from Sheffield at eight or ten-pence a pound, diſpatched their orders thither, without any regard (which is too common a caſe) to the trouble and expence, which at their own requeſt I had been at, to ſerve them under their difficulties. What advantage this exchange hath been of to our London artificers, I ſhall not pretend to determine; they beſt know, or ought to know, whether it hath been of any, or the contrary. But certainly, the diſcovery of this affair at Sheffield hath turned out of great ſervice to that large and populous ſeat of manufacturers; which wears, as I have been well informed, at preſent, a very different aſpect from what it did not many years ſince, before it was in poſſeſſion of this new and valuable branch of commerce.

Thus have I given a brief hiſtorical account of this recently diſcovered art of reducing bar-ſteel by fuſion, into a more compact and uniform texture.

As to the phyſical nature of the change wrought upon this uſeful metal by the new manner of reduction; I ſhall be but ſhort upon it; and perhaps not quite ſo explicit, as ſome perſons, for their own intereſt, may wiſh me to be.

It has been obſerved in the beginning of this eſſay, that no longer ago, than when Mr. Reaumur publiſhed his treatiſe upon converting bar-iron into ſteel, which method of coming at that valuable metal he juſtly prefers to every other; I ſay at that time, this new method of operation was judged to be very difficult; nay, he pronounces it abſolutely impracticable. I own, I was then ſo far of his opinion, that I ſhould ſcarce ever have ventured upon ſuch an attempt, if the practicability of the operation had not been made apparent by its having actually been effected by the perſon I before mentioned, and if I had not likewiſe been ſtrongly ſolicited to make the experiment.

What might poſſibly lay a foundation for the opinion of its being impracticable, might be the accident, before taken notice of, which ſometimes happens at the converting-furnaces; when by ſome imperfection in the pots, or cheſts, the groſs air finds a paſſage in among the contents; which is ever ſure to demoliſh a conſiderable quantity of the metal, if running it down into groſs lumps may properly be called a demolition.

Some years ſince, a very ſenſible man fell in my way, who had many years been employed at Mr. Crawley's works at Newcaſtle; this perſon informed me, that accidents of this nature fell out, not unfrequently, at their ſteel-furnaces in that place; and that great quantities of this reduced metal, which he had ever obſerved to be as hard and obdurate as pig-metal itſelf, had been buried in the earth to prevent a diſcovery, leaſt the workmen employed in this part of the works might ſuffer on account of their inadvertency. If the relation given by this man was really true, it is great pity the fraud was never diſcovered, and a recovery of the metal attempted; as it might eaſily have been reſtored by a little trouble at the Finery, and made into a better metal, either in the form of iron or ſteel, than before the accident happened. But to return:

With regard to the phyſical difference between converted bar-ſteel, and that which hath been reduced from the bar by fuſion, it conſiſts principally in the change, which is made by the different arrangement of the atoms, which compoſe the metal; in the former caſe, after it is taken out of the converting-furnace, when broken, it uſually exhibits to the eye, (if the iron made uſe of has been properly choſen,) the appearance of large ſhining maſſes; in the latter, there appears only a kind of a groſs grainy texture; with this eſſential difference between the two, owing to their different forms, that, though when they are reduced into rods, and their pores cloſed by the hammer, and then properly hardened, when broken they ſhall each of them diſcover an equally fine grain; yet, ſuffer them to be hammered into plates, or any flattiſh inſtruments, as razors for inſtance, and when theſe are properly hardened and tempered, and after that finely poliſhed, place them horizontally between your eye, and the light, and you will eaſily diſcover a very manifeſt difference; for while the former ſhall appear in this ſituation, uneven and ſcurfy, no poliſhed glaſs can diſcover a finer or more even ſurface than the latter. Or, let them be reduced into a proper form for reflecting-mirrors, whether plane, concave or convex, and treated as in the former caſe, and the difference in their appearance will exactly and uniformly correſpond to the former inſtance. I had the pleaſure of making ſeveral of theſe mirrors for the very ingenious Dr. Ingenhouſt, while he was laſt in England, which with ſome difficulty I found a method to get hardened, and then had them neatly poliſhed, much to the Doctor's ſatisfaction.

But with regard to the uneven and ſcurfy appearance above-mentioned, I apprehend that to be owing to ſome remaining quantity of foul ſulphur not thoroughly purged out, and hindering the atoms from coming into proper contact; for let the bars of iron be hammered down with ever ſo much care and accuracy at the Chafery, and even after they are converted into ſteel, they will yet be liable to many large bliſters formed on their ſurfaces, as well as in the interior parts, by the ſaid ſulphur: but when the bars of ſteel are reduced by fuſion, the foul ſulphur is ſo far deſtroyed or driven out, that the atoms of the metal are permitted to come into cloſe contact, and ſo exhibit a ſmooth uniform ſurface.

How theſe circumſtances may affect the edges of fine cutting inſtruments, is a matter of ſtill farther enquiry. One would naturally imagine, that while their ſurfaces are liable to ſuch irregularities, it ſhould be next to impoſſible to preſerve their edges entirely free from them. Thus much I apprehend we may ſafely infer, that the ſteel, which is remelted from bars of converted iron, muſt ſtand a much better chance to produce inſtruments more pure, better connected in their different parts, and conſequently not ſo ſubject to the above-mentioned inconveniences, as when they are formed directly from bars juſt taken out from the converting-furnace.

After all, there is ſtill much more knowledge and experience requiſite, than moſt people are aware of, to produce fine cutting inſtruments, even though the method of reducing ſteel by fuſion has now for ſome years been known, and put in practice.

I laſt ſummer took a journey to Sheffield, to try whether it might not be poſſible to have the ſteel melted there, upon ſuch terms, as to enable me to reduce the price of my razors, &c. but in this I found myſelf greatly diſappointed; for though I took with me ſteel of the moſt excellent quality, converted for the very purpoſe; though I prepared, and carried with me, a ſufficient quantity of my own flux, and had the ſteel melted under my own immediate inſpection; I ſay, notwithſtanding all theſe precautions, when the ſteel came to be wrought into razors, and other inſtruments, by one of the ableſt artificers I could procure, they fell ſo far ſhort of thoſe made from the ſteel I had before melted, and wrought down to proper ſizes, at my own manufactory *, that I could not honeſtly diſpoſe of them upon any terms.

Having

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* The ſteel-manufactory in White-Croſs-Alley in Middle Moorfields, where the beſt razors, lancets, &c. made by the author, may be had; as alſo at Mr. Thomas Wilkinſon's, at his warehouſe for hardware, &c. No. 75. over-againſt St. Michael's church in Cornhill. Were the author inclined to ſacrifice to his own vanity, he might mention the approbation and encouragement he hath received from many great perſons, not excepting the greateſt in the kingdom; but he hopes to be excuſed in relating the following circumſtance, that when the very ingenious Dr. Ingenhouſt was laſt in England, after having been for ſome time in queſt of razors peculiarly excellent, and almoſt every where diſappointed, he was by my late worthy friend Dr. Gowen Knight furniſhed with my addreſs, and ſending me a card, I took with me two dozen, and left them with him for his choice upon trial. After ſome time he informed me, he was quite tired with trying the razors, as he could not find one indifferent one amongſt them all: and accordingly he purchaſed at my own price the whole number. It is with pleaſure I recollect the marks I received afterwards of the Doctor's politeneſs and friend-

Having ſaid ſo much upon the firſt part of Mr. Reaumur's work, relative to his method of converting iron into

ſteel;

friendſhip, and particularly, the ſeveral letters he honoured me with, when at Sheffield, containing a number of curious enquiries, as well as orders for various kinds of inſtruments, which as I executed with pleaſure, ſo I had afterwards the pleaſure to know, that it was to the perfect ſatisfaction of my ingenious correſpondent.

It may not perhaps be amiſs, if I take an opportunity in this place, of laying down a few rules for the management of razors; and very few may ſuffice.

After every operation, the razor ſhould be gently applied to the ſtrap, in the following manner:

The razor ſhould be held in the hand in ſuch a ſituation as to prevent its turning on the rivet. This may eaſily be effected by holding it at the ſmall end of the ſcale, in ſuch a manner as to have ſecure hold both of the ſcale and the blade. Then let the razor be applied quite flat to the ſtrap, ſo as that the back and the edge may both reſt upon the ſtrap; for if the blade be ſuffered to form almoſt the leaſt angle with the ſtrap, there will be

danger

ſteel; I ſhall, before I conclude, ſubjoin a few thoughts as to the latter part: Entitled, *L'Art d'adoucir le Fer fondu,*

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danger of turning the edge; which will for the time render it utterly incapable of performing its office. In this caſe the only method proper to reſtore it, is to draw the edge gently two or three times over a little piece of horn, which generally is, or ought to be, placed at the end of the ſtrap. This will deſtroy, or break off this feather-edge, as it is called: which is often performed by the barbers by drawing it over the nail of their thumb. Then give it a few ſtrokes afreſh upon the ſtrap, which will certainly reſtore it.

Many of my friends have aſſured me, that by ſtrictly adhering to the rules above-mentioned, they have kept their razors in good order for ſeveral years, without ever having occaſion to uſe the hone. One gentleman within theſe few days informed me, his razor had been uſed for ſeven years, without having ever touched the hone. Notwithſtanding which, I think it would be proper now and then to have recourſe to the hone, as it certainly hath a tendency, if it be done but gently and ſeldom, to keep the edge of the inſtrument in more compleat order.

The Art of nealing or ſoftening caſt Iron; as to which however very little need be ſaid, as I have reaſon to apprehend, the ſucceſs in that attempt did by no means anſwer his expectations; nevertheleſs, to ſatisfy the curioſity of ſome of my friends, I ſhall venture to lay before them what I know of the matter, having tried in every ſhape the conſequence of Mr. Reaumur's doctrine. His reaſoning is as follows: viz. If impregnating with a ſufficient proportion of ſalt and ſulphur will turn iron into ſteel; of a higher or lower degree in proportion to the quantity of materials proper for the purpoſe, which are made uſe of, and to the time employed in the operation; then, if I can find matter of a contrary nature, which by being poſſeſſed of a ſufficient abſorbing quality, will draw back and drink up the oppoſite matter, by the uſe of this you may rob the ſteel entirely of its ſteely quality, and reinſtate it in its former condition, of being nothing but ſimple bar-iron. To aſcertain the truth of this hypotheſis, he, as uſual, made uſe of a variety of materials, and went through a great number of experiments; at length he ſeems to have pitched upon calcined bones of animals, reduced to powder, as the baſis of his hopes of ſucceſs in this undertaking. It is well known that theſe bone-aſhes, as they are called, are the matter made uſe of by the refiners, to form their teſts, for refining their gold and ſilver; and as the medium employed for that purpoſe is lead, the quality of which medium is to diſſolve all the impure metal with which the pure metals happen to be mixed, and to carry it off in fume; that part of the impurity, which is not thus carried off by ſublimation, is left to be abſorbed by the bone-aſhes which form the teſt. This conſideration was beyond diſpute a proper foundation on which Mr. Reaumur might expect to erect his ſuperſtructure. But upon a farther proſecution of the affair, he ſoon became convinced, that the material he had pitched upon was not of a proper nature to anſwer his purpoſe; for though the matter to be wrought upon, was not common ſteel, but pig-metal; which it has been obſerved is ſteel of the higheſt degree, yet ſo powerful was the material, that at the ſame time that it extracted the ſalt and ſulphur, it likewiſe by the help of the fire neceſſary to be made uſe of in this operation, ſo far calcined the metal as to render it a mere crocus of iron. In order to remedy this inconvenience, he was under a neceſſity to abridge and correct its power, by mixing with it a certain proportion of charcoal powdered; as follows, to two parts of calcined bones, one part of powdered charcoal. This mixture ſo far anſwered his purpoſe as to reduce the metal. But as his ultimate intention was to form fine ornamental works from pig-iron, and then after reduction by theſe materials, to have them repaired and poliſhed, here he miſcarried; for though this method would extract the ſulphur and ſalt from the caſt iron, yet in producing this effect, it occaſioned ſuch a number of pin-holes, or ſmall vacuities on the ſurface, as rendered his works utterly incapable of being poliſhed to any tolerable degree of accuracy.

I have thus laid before my reader, a ſhort but faithful narrative of his whole ſcheme. If any one ſhould have an inclination, to try any farther experiments upon this bottom, he may ſafely purſue his deſign from the hints here diſcovered, without giving himſelf the trouble to peruſe the whole performance.

I have myſelf tried the truth of the hypotheſis in a rough way, as far as nealing, or merely reducing, has any concern in the matter, by a great number of experiments; two only of which I judge neceſſary to recite by way of ſpecimen. I had ſeveral ſmall ingots of caſt iron, about three quarters of an inch thick; theſe I put into a crucible, covered all round with the matter above deſcribed, and in that ſituation committed them to the fire, where I ſuffered them to remain a ſuffia ſufficient time, then took them out, and gave them a ſtrong heat; when the exterior parts were become capable of bearing even a welding-heat, while the interior ſuffered an eaſy diſſolution, and ran out into the fire. I ran ſome other ingots in the form of piſtol-barrels, which having treated in the ſame manner, the ſame conſequence enſued; the exterior caſes as before remained firm and ſtable, while the interior, diſſolved and ran out, and left caſes in the entire form of piſtol-barrels.

And now on the whole, I think I have not made any material omiſſion in the proſecution of my deſign; I only wiſh the ſubject had been treated with a greater degree of accuracy. To elegance of ſtile I make no pretenſion; neither does the ſubject require it; embelliſhments of that kind are by no means requiſite, where a faithful narrative of facts, founded upon cautious and well-tried experiments, is the principal thing in view. I may poſſibly be charged, and perhaps not altogether unjuſtly, with having here and there been guilty of tautology; all the apology I have to make, is, that the performance has been in a great meaſure compiled from old papers, written at different times; in reviewing which, in order to reduce the ſcattered hints into ſome reſemblance of a regular ſyſtem, it is next to impoſſible, that the ſame ideas ſhould not ſometimes have occurred again; and it might not be quite improper to repeat them, in order to impreſs them the more ſtrongly on thoſe perſons, for whoſe benefit I am chiefly writing. Poſſibly I may be thought, in ſome few inſtances, to have dealt too freely with the character of the very celebrated Mr. Reaumur; a character I extreamly revere, particularly as he was the firſt who attempted to reduce the affair of converting iron into ſteel to a regular intelligible ſcience. I wiſh he had been more ſucceſsful in the attempt; but he appears to me to have met with the common fate which attends the undertakings of all gentlemen in caſes of this nature, who are obliged to place their chief dependance upon ſuch information as they are able to collect from workmen, many of whom are ignorant, and others deceitful and impoſing. Mr. Reaumur himſelf very pertinently obſerves the ignorance in this affair even of the great Rohault, and intimates that he could name an hundred others in the ſame circumſtance.

cumſtance. And I have more than once had occaſion to remark, in the courſe of this brief eſſay, how little dependance is to be had on moſt of our lexicon, or dictionary writers, in ſubjects of this nature; I venture again to offer it as my opinion that Dr. Harris, ſo far as I may be thought a competent judge, greatly excels all his ſucceſſors in that way of writing; at leaſt he deſerves that character, in his judicious hints relative to the ſubject before us. But it ought to be obſerved, that Mr. Reaumur wrote his treatiſe in a great meaſure to be an amuſement for gentlemen, and the appearance of his book entitles it to a place in the genteeleſt library; it is finely printed, the draughts contained in it are inimitably executed; but when you come to examine the vaſt number of experiments he chooſes to mention, I am greatly miſtaken, if you do not find many of them extreamly trifling, and of ſuch a kind that no reaſoning, *a priori*, from the nature of the ſubject he was upon, could by any means have led him into. And I do not remember to have met with a ſingle inſtance, where an artiſt in the ſteel-way has ever profeſſed to have received any benefit from his writings. It is now fifty years ſince his elaborate treatiſe made its appearance, during which time, ſuch have been the improvements in ſcience, particularly in magnetiſm, electricity, &c. as has occaſioned ſtricter enquiries into the nature of iron, and ſteel, eſpecially the latter, ſo that gentlemen of education and ſcience are now not ſo eaſily brought to ſit down ſatisfied with mere theoretic, and plauſible hypotheſes; but require ſuch evidence as will ſtand the teſt of the ſtricteſt experimental ſcrutiny. As I ſet out with a profeſſion, not barely to amuſe this ſort of ſcientific readers, but likewiſe to free them from wrong preconceived opinions too haſtily taken up, as well as to ſecure them againſt ſuch miſapprehenſions for the future; I flatter myſelf I have not fallen very far ſhort of my intention. However, I was at the ſame time deſirous, not only to approve myſelf to ſuch gentlemen, by my endeavour to aſſiſt them in their laudable enquiries; but alſo to benefit thoſe of my friends, and others, who are actually engaged in works of this nature, either in a more extenſive, or contracted way; and I hope this little performance will not prove without its uſe to them; as I am pretty certain there are here and there a few hints to be met with, which have never occurred in any written accounts before; at leaſt not in any one, that has ever fallen under my obſervation.

# APPENDIX,

DISCOVERING

A more perfect Method of Charring PIT-COAL, ſo as to render it a proper Succedaneum for Charred WOOD-COAL.

# APPENDIX.

MANY attempts having been made to find out a proper method of reducing pit-coal, in ſuch a manner as to render it a proper kind of fuel for the reduction of the iron-ore at the ſmelting-furnace, as well as for other operations afterwards at the other furnaces; and even at the various forges of the working cutlers, where a clean pure fire is always attended with peculiar advantages; and this matter ſtill continuing a deſideratum of the utmoſt conſequence, I have made the accompliſhment of it my particular ſtudy; and in the proſecution of it have ſpared neither pains nor expence.

As a foundation for this attempt, I found it neceſſary to be well aſcertained, what part of this uſeful mineral was requiſite to be diſcharged, as entirely noxious; and what part might be retained, as uſeful and ſalutary.

For this purpoſe, a few years ſince I ſpent ſeveral months at a friend's in Shropſhire, where I was informed ſeveral of theſe attempts had been made, and where at that time great quantities of this kind of fuel were made uſe of, in order to run down pots and kettles, as well as larger works, ſuch as cylinders for fire-engines, &c. Among other information which I was able to pick up in that part of the country, I met with a tradition that a famous Dutchman had made his appearance in that neigbourhood many years ago, under the character of a great connoiſſeur in the ſmelting of iron-ore with pit-coal: this perſon being thought to have a conſiderable ſhare of knowledge in this way, his propoſals were eagerly embraced by ſome of the iron-maſters; and no pecuniary aſſiſtance was refuſed him, in order to put his ſcheme in execution. A very coſtly furnace was erected for the purpoſe; much more ſo, than it need to have been, had the projector went upon true and genuine principles. But unhappily, he had ſome how or other imagined, that the flame iſſuing from the fuel, becauſe it put on a clear and ſplendid appearance, muſt be pure enough, could it be conveyed to the ore with ſufficient ſtrength, to be a proper diſſolvent of it. To effect this, (as far as I could procure any tolerable account of the ſtructure of the furnace, which for many obvious reaſons muſt have been a draught one,) the flame was by various turnings and windings to have been ſo urged on, and directed by the force of the air, as to terminate in ſtrong and powerful focuſes againſt the body of the ore; ſo as to have procured a diſſolution, without any of the groſs parts of the fuel coming in contact, or having any immediate buſineſs in the operation.

Had the Dutchman better underſtood the nature of our mineral fuel, he would never have ſuppoſed, that the flame could by any means have furniſhed a pure or proper diſſolvent of the iron ore; his ignorance in this reſpect defeated his project, as the metal ſeparated by this method diſcovered ſuch extreamly bad qualities,

lities, as rendered it abſolutely unfit, even for the moſt ordinary purpoſes. This ill ſucceſs ſoon iſſued in the breaking up of the ſcheme, and the demolition of a very curiouſly conſtructed furnace.

Whether this miſcarriage furniſhed the firſt hint, of the neceſſity of charring the pit-coal, in order to render it uſeful, by carrying off the phlegm, and with it the more crude and hurtful part, of the raw and undigeſted ſulphur, I am unable to determine. But it is a fact now well known, that the method generally made uſe of for this purpoſe, is burning the coal to a cinder, or turning it into what they call coke.

This operation is performed in various ways. Moſt of what I ſaw burning for this, and other purpoſes, during my ſtay for ſeveral months in Shropſhire, was performed in the following manner:

They raiſed a pretty large circular hillock of raw coals upon the ground, covering them over with earth, only leaving a ſmall paſſage open, for the convenience of ſetting fire to the heap; and the whole heap being ſet a burning pretty ſtrongly, they then ſtopped up the before-mentioned opening, with every other creviſe they could poſſibly diſcover, to prevent any acceſs of the common air; which, however, ſometimes, notwithſtanding all their precaution, forced an entrance. Whenever that happened, the coals inſtead of being burnt to a cinder, were reduced to a heap of aſhes.

Such a proceſs anſwers the end extreamly well for the burning of peat, where the aſhes only are wanted, as a valuable manure for land.

In the villages round about the metropolis, where this ſort of fuel is prepared for the uſe of the malſters, and ſome few other purpoſes, they purſue a very different kind of management. They burn the raw coals in ovens erected for the purpoſe, in ſome convenient open place. Theſe ovens being from time to time, charged with a proper quantity of coals, they ſet them on fire. Near the front or opening of theſe ovens the chimnies are placed; at which outlets, when the coals become ſufficiently ignited, the flames, which play round the interior parts of the oven, make their exit, carrying along with them a very conſiderable part of the crude ſulphur.

It may not be amiſs to obſerve here, that the coals made uſe of for this purpoſe at, or near London, are of a very different nature, from thoſe which are dug up in Staffordſhire, Yorkſhire, and moſt of the northern counties, which abound with mines of iron-ſtone. The coals burnt about London, for making cinder, are generally, if not always, ſuch as are brought from Newcaſtle; which as they are not ſo eaſily reduced to aſhes, but are apt, very frequently, to turn to obſtinate vitrified clinkers, are by no means proper to be made uſe of for reducing iron in ſmelting-furnaces, to form pig-metal, tho' they may ſerve for ſome inferior purpoſes. But ſuch coals only are fit to be made uſe of for the above intention, as are eaſily conſumed, leaving nothing behind but pure clean white aſhes.

But I ſhould have obſerved, that where theſe ovens are made uſe of, the workmen employed at them, when they imagine the coals are ſufficiently burnt, draw them out with an iron raker, upon the ground before the oven, where they endeavour to ſtifle the yet remaining part of the ſulphur, by quenching them with a deluge of water. Thus they go on charging, diſcharging and ſuffocating, till they have compleated their intended quantity.

But as neither this method, nor that which I had ſeen practiſed in Staffordſhire, could upon the ſtricteſt experimental ſcrutiny be brought fully to anſwer the wiſhed-for purpoſe; I employed my imagination, to find out, if poſſible, the eſſential reaſon, why wood-coal charred ſhould effectually anſwer ſo many valuable purpoſes, while pit-coal charred could ſcarcely be brought to any tolerable diſpoſition, as a ſuccedaneum, to anſwer ſimilar intentions. After having tried no inconſiderable number of experiments, and revolved the affair over and over, I at length imagined at leaſt, that I had hit upon the true cauſe.

I was fully perſuaded, with regard to pit-coal, as well as wood or any other materials fit for fuel, that after the more hurtful, and corroſive part of the ſulphur was, in the ſmoake and flame, ſufficiently carried off, if the remaining leſs noxious, and leſs voracious ſulphur, could be brought into a ſtate as fixed and permanent as that in the charcoal, I ſhould become poſſeſſed of the wiſhed-for deſideratum: And the fixing of this remaining ſulphur, I apprehended, might be effected by the burning coal being reverberated in a cloſe furnace or oven, till all motion ſhould ceaſe, and the fire, as we expreſs it, go out of itſelf.

At this time, a gentleman with whom I was very intimate in Staffordſhire, informed me that there had lately been diſcovered in that neighbourhood a rich vein of iron-ſtone, from which great profit had been expected; but that upon trial, it had produced only a very bad kind of red-ſear iron; and that by attempting to make uſe of it, in mixture with other mine-ſtone, it had communicated the ſame bad quality to the whole maſs. As I thought, that trying the effect of pit-coal charred according to the method I propoſed, upon ſome of this very bad mineſtone, would, if it ſucceeded, be reckoned a deciſive proof of the utility and excellence of pit-coal ſo charred, I acquainted my friend with my purpoſe, and made him the following propoſal: That if he would be at the expence of the materials, find a proper place, and employ an expert workman, to erect ſuch an oven, as I ſhould direct, I would take upon me the trouble and expence of ſending down a fit perſon, to ſee the whole executed, and the experiment properly made.

I ſent down accordingly my own ſervant, who had for many years accompanied me in all my experiments, one in whoſe judgment and fidelity I could place the utmoſt confidence; who having received full inſtructions, the building was properly erected, and made uſe of with all the ſucceſs that could be wiſhed for; a ſufficient quantity of the coal was prepared in it, which was left as above hinted to expire, and die away of itſelf, after the main part of the flame with the crude ſulphur had been conveyed up the chimney, and the mouth of the oven cloſely ſhut up and luted, to prevent any acceſs of the common air.

But before I proceed to enumerate the many advantages reſulting as the iſſue of this experiment, I apprehend it cannot be amiſs, to apprize thoſe of my readers whom it may more immediately concern, of ſome of the difficulties which occurred in the proſecution of it.

As I had been all along ſuſpicious, that the alternate elevation, and ſuppreſſion of the ſulphurous vapour, notwithſtanding ſo large a portion of it had been carried off in form of flame, would of neceſſity, when the mouth of the oven came to be cloſely ſhut up, ſo as to prevent the leaſt acceſs of the external air, or eſcape of the remaining ſulphur, occaſion ſuch an expanſion as might endanger the blowing up of the oven; for this reaſon, I deſired my ſervant to take all imaginable care in the conſtruction of the oven, or furnace, and more eſpecially that the crown of it might be made ſufficiently thick and ſtrong, capable of reſiſting all oppoſition which might poſſibly be raiſed againſt it. In the courſe of the operation all this precaution appeared to have been neceſſary; for notwithſtanding thoſe orders had been punctually attended to, yet they were obliged to lay a large additional weight upon the crown, in order to be fully ſecured againſt the apprehended danger.

This trial however, notwithſtanding the few difficulties that attended it, turned out as ſatisfactory as could have been expected, or even deſired. During my ſervant's ſtay upon the ſpot, he recommended the trial of this fuel to ſeveral ingenious artiſts, and among the reſt to ſome in the jewelling way, who were hardy enough to make uſe of it in ſoldering ſome of their curious work; for which purpoſe they declared it anſwered the end as well as the beſt charred wood-coal they had ever met with.

Conſidering the matter as of great importance, I determined to make all the experiments I could think of, in order to aſcertain the properties of this new ſort of fuel. I therefore procured a ſufficient quantity of the bad mine-ſtone before mentioned, and with it ſome of the coke or cinder, of both ſorts; I mean ſome that had been prepared in the common method of burning, and ſome that had been cured by burning in the oven I ordered to be erected for the purpoſe. Having roaſted a quantity of the mine-ſtone, to render it fit for diſſolution, I put a part of it reduced to powder, into a crucible, mixed up with a due proportion of the firſt ſort of coke, powdered likewiſe, with ſome of the common flux: when this mixture came to feel a ſufficient degree of heat from the fire, a ſeparation was effected, without any extraordinary trouble; the operation, as I expected, produced a very bad red-ſear kind of iron, quite rotten, and of no value. After this I mixed up, in like manner, a freſh parcel of the mine-ſtone, with ſome of the coke that had been prepared according to my direction; without any peculiar degree of caution, except an abſolute rejection of the minuteſt quantity of lime-ſtone in the flux; inſtead of which I made uſe of a very inoffenſive flux, as cheap and as eaſy to come at as lime-ſtone, which I have ſufficiently hinted at in the foregoing eſſay. This reduction of the ſame mine-ſtone turned out beyond my moſt ſanguine expectations. The metal at the bottom of the crucible was ſo far from being a red-ſear rotten iron, that in fact it was a very fine grained ſteel;

ſcarcely malleable indeed, but ſuch as with a proper degree of caution ſubmitted to the ſtroke of the hammer, ſo far as to bear beating out to a point, and afterwards underwent pretty kindly the proceſſes of being hardened and tempered. In ſhort, the coke, managed in this method, I tried in every ſhape I could think of, even for converting iron into ſteel; and, in every inſtance, it appeared ſo apt and proper a ſuccedaneum for charred wood-coal, that, I own, I ſhould be at a loſs which of the two to prefer for any very curious purpoſe.

It may here poſſibly be enquired, how ſo valuable a diſcovery came to be neglected, eſpecially in a part of the country where ſo much buſineſs in the iron way was carried on? This was owing to my being in ſome ſort connected here with two perſons of very different qualifications; one an expert iron-maſter, the other only a country attorney, a little fond of dabbling in things which he did not well underſtand. Juſt when matters were ripe for excution, the former died; the other afterwards appearing to be a perſon, one would not wiſh to be concerned with, the affair was for that time laid aſide.

It may likewiſe be objected, that an oven, or furnace, fit for the purpoſe, it ſhould ſeem from what has been hinted, would be liable to many accidents and inconveniences; and that a great number of ovens would be requiſite to ſupply a ſufficient quantity of fuel for a large iron-work.

Theſe difficulties, I apprehend, are ſcarcely worthy of notice, provided the certainty of the doctrine laid down be well aſcertained. The very firſt attempt, though attended with a few trifling difficulties, turned out in every reſpect ſo ſatisfactory, that there can be no doubt, a workman ſkilled in erections of this nature may be able to improve upon the ſcheme, by erecting ſuch an oven, or ovens, as may be better adapted to anſwer the purpoſe, than that in which our firſt attempt was made.

As to the number which might be requiſite to ſupply a large work, I would aſk leave to interpoſe a query: Might not an oven be ſo contrived, as to have behind it, by way of reſervoir, a vault ſufficiently capacious to contain the quantity of fuel that might be prepared by fire, ſix or more burnings in the oven? into which vault the fuel, when duly prepared, might be diſcharged from time to time from the oven under ſuch precautions, that when ſo diſcharged, the paſſage from the oven to the vault might be ſtopped. And as there muſt of neceſſity be an aperture ſufficient to draw out the coke from the vault, that too muſt be properly ſecured againſt acceſs from the external air. But after all, no great difficulty or expence can attend the building of a ſingle oven, in order to try the experiment: and if the operation be duly performed, according to the precautions laid down, I will venture to prophecy, that the event will exceed expectation, and certainly ſupply a ſuccedaneum, that will anſwer every purpoſe for which charred wood-coal can poſſibly be wanted.

After I had proceeded as far in this operation as is above related, upon an interview with my old ſervant, he informed me, that it would be abſolutely neceſſary, in order to prevent the blowing up of the furnace or oven, to leave ſome outlet in the top of the crown, that ſome part of the vapour might be ſuffered to make its eſcape, when the expanſion happened to be too powerful for the building to ſuſtain it: while, at the ſame time, it would be as neceſſary to prevent ſuch eſcape as much as poſſible; ſince the greater the quantity of this fine ſulphur, which could be brought to a ſtate of fixation, the better the fuel would be to anſwer the purpoſe. He was of opinion, that this might be effected either by a proper valve, ſuch as is made uſe of in the ſteam-engines; or by ſome other ſimilar contrivance. As they had made no proviſion of this kind in the firſt furnace, of which he had the direction, they were ſubjected to great inconveniences; for the ſtrength of the vapour ſeveral times forced its way through little breaches it had made in the building, which they were under a neceſſity of repairing in the beſt manner they could. As the ſtrength and force of the vapour declined, they took particular care to prevent any farther eſcape as much as poſſible, till at the latter part of the operation, they had brought their incloſure to form an abſolute occluſum.

After all theſe miſhaps and inconveniences, my ſervant aſſured me, that the fuel procured even in this imperfect manner, anſwered the purpoſe, in all the trials he had made of it, for reducing the mineſtone, much better than any charred wood-coal he had ever made uſe of; for he found the metallic atoms mixed in the ſtone, much eaſier collected by it, and likewiſe that it entirely prevented the corroſive ſulphur of the mine from corroding his crucibles, which common charcoal would not always do. This laſt-mentioned advantage I cannot but look upon as a very peculiar one, ſince many uſeful corollaries may, I think, be drawn from it.

I have lately had it hinted to me by a gentleman of good underſtanding in the nature of manufactures and commerce, as a thing much to be deſired, that this method of curing pit-coal might be rendered ſerviceable to our woolcombers. I apprehend that this might eaſily be effected; at leaſt, the trial of it might be made at no great expence, as a ſmall apparatus would be ſufficient for that purpoſe.

THE END.

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